United States Air Force
Scientific Advisory Board

Report on

Space Surveillance, Asteroids and Comets, and Space Debris

Volume 2: Asteroids and Comets

SAB-TR-96-04

June 1997

Authorized for Public Release - June 1998

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Executive Summary

This study, *Asteroids and Comets,* is the second part of a threefold study, *Space Surveillance, Asteroids and Comets, and Space Debris,* that was requested by Commander, Air Force Space Command, and approved by the Secretary of the Air Force and Chief of Staff of the Air Force.

This Executive Summary provides an overview of the Committee's findings on asteroid and comet impact warning for Earth and a summary of the Committee's recommendations.

Situation. Near-Earth objects (NEOs), asteroids and comets, hit the Earth relatively frequently on geologic time scales. Their frequency of impact is roughly inversely proportional to the square of their diameter. The largest NEOs, which approach 10 km across, produce global effects and massive numbers of fatalities; however, they impact relatively infrequently. The last recorded impact was the 8-km asteroid whose impact at Xhichulub on the Yucatan peninsula 58 million years ago is thought to have ended the age of the dinosaurs. NEOs 1 to 2 km across can also cause global damage and extinctions, and they do so about an order of magnitude more frequently. The recent impact on Jupiter of comet Shoemaker-Levy was of this size and produced large effects over a region the size of the Earth. It was this impact that energized the scientific community and the U.S. Congress to take action in this area.

The distinction between the largest (10 km) and large (1 km) NEOs is imprecise, mostly having to do with the size of NEO that can produce phenomena that can propagate to all regions of the globe. The criterion for global effects is not now known with any precision, but it could be on the order of 1 km. Thus, all NEOs larger than 1 km are treated together below.

Intermediate-size objects (0.1 to 1 km) probably do not cause global effects, but they can produce regionally devastating phenomena such as blast waves, widespread fires, and tsunami, which they apparently do every few thousand years. Tsunami, deep-water waves caused by mid-ocean impacts, are particularly damaging in that they steepen as they reach shore. Thus, they can cause flooding tens of kilometers inland in just those agricultural areas and harbors where population and value tend to concentrate. And because they can produce such waves all the way around ocean basins, they have the potential for massive regional damage.

Small objects (10 to 100 m) cause local fireballs, blast, and destruction on the spatial scale of cities and states, and they do so on the time scale of months to centuries. The breakup of even the smallest objects (1 to 10 m) can be mistaken for nuclear explosions, presenting false alarms for missile warning systems.

Characterization. Large NEOs are thought to be well characterized, even though it is estimated that only about 10 percent have actually been discovered to date. Most of those discoveries have been through century-long search programs with film-based telescopes. Planned programs to increase that discovery rate generally involve decadal searches using conventional optical telescopes with improved electronic focal planes. As, on the average, most NEOs should pass close to the Earth a number of times before hitting it, such searches are an effective way of using duration of search to minimize the technical performance required from the telescopes and focal planes used for it.

For long-period comets (LPCs), objects with periods greater than 220 years, decadal search does not appear to be adequate. Most LPCs would not have been observed yet. Indeed, many of them might be observed only on their final pass toward collision with the Earth. The composition of the LPCs is at present poorly known and quite controversial. LPCs might make up anywhere from 4 to about€40 percent of flux of objects, with the higher values coming from some of the most accomplished comet observers. This uncertainty is compounded by a corresponding uncertainty in the reflectivity or albedo of the LPCs, which causes them to appear as uncountable "stealth" objects in some analyses. LPCs could roughly double the number of objects in the threat, but more importantly, their observation on their final pass would require response times on the order of months to years rather than the decades to centuries thought to be available for other types of NEOs. Thus, LPCs of all sizes are the most stressing class of NEOs because of the strong challenges they present to both detection and interception schemes.

The effects of intermediate NEOs are not as well documented—in part because their fossil record is transient. It is known that geologic activity in the seabed can cause devastating tsunami. It is less clear that ocean NEO impacts have done so. At present, the connection is based on the anomalous location of coral and other seabed materials, anecdotal evidence, and careful theoretical analysis of deep-water and runup phenomena. The effects of onshore damage have been studied only parametrically. The effects of small objects have been documented more carefully, as they occur more frequently.

Impacts over Tunguska in Siberia in 1908, the Brazilian rain forest in the 1930s, and elsewhere have made it possible to produce a sound body of evidence and interpretation as well as a thorough assessment of the impacts of the objects, which are largely analogous to those of the explosions of nuclear weapons in the atmosphere. The smallest objects have been studied scientifically, because meteors strike the Earth's atmosphere continually. More recently, the information from defense warning satellites has also been made available to that scientific data base, in part to aid interpretation and in part to seek means to reduce false alarm rates.

Detection. Technology for ground-based telescopes is rapidly shifting from film to charge-coupled device (CCD) focal planes. Although film has long been a convenient and compact storage medium, its sensitivity is far below that of the human eye. Thus, the CCD, essentially a digital, silicon retina with electronic processing and readout, is a natural for a low-light, wide-area search such as NEO detection. The possibility of shifting to CCDs has been studied a number of times over the last decade, and the Air Force Spacewatch now mounts a limited CCD search. The NEO Survey Working Group commissioned by Congress in 1995 recommended that megapixel CCD focal planes on existing telescopes should be the most effective sensors, and that a single such telescope costing about $25 million could perhaps complete the search for large NEOs in a decade. This search entails a large shift in strategy. In the past telescopes increased aperture to maximize the distance to which they could see. Now they increase their detector count to maximize their field of view and rate of search, which permits greatly increased performance from existing telescopes.

Rapid ground-based search for large NEOs is only part of the problem. It is also necessary to search for LPCs, as well as dimmer and smaller objects that might be seen only on first approach. For them it is useful to shift the sensors to space, where the sensors can take advantage of unobstructed viewing and take advantage of the favorable scaling on both aperture and focal plane size to produce compact and efficient sensors that can see out to the fraction of an astronomical unit (AU, the distance from the Earth to the Sun) required for adequate warning and response. A number of such sensors in constellations around the Earth could provide advanced detection as well as almost any warning requirements.

Deflection. Although the committee concentrated on the search for asteroids and comets, it would be appropriate to comment on the deflection of these objects from trajectories that might impact the Earth. Given detection, it should be possible to deflect most NEOs away from the Earth. If an inert or explosive payload were delivered to the NEO, its impact would blow a greater amount of material off the NEO, whose removal would cause the NEO to move in the opposite direction. If the deflection were great enough, the NEO could miss the Earth. Even if the NEO were too large to be deflected completely away, the impact could still pulverize the NEO into pieces too small to survive atmospheric entry. This pulverization option is interesting because it has the potential to reduce the energy and cost of deflection by about a factor of 100. Indeed, pulverization with kinetic energy interceptors should be possible for NEOs of diameter up to about 0.5 km, which includes the most frequent impactors. The only modification for LPCs is that the interceptors would have to be maintained in readiness. Once such interceptors were developed, they could also be retrofitted with higher-energy explosives, if required for larger objects.

It would be difficult to achieve the conditions for deflection in terrestrial experiments. Fortunately, asteroids frequently pass the Earth in convenient trajectories that are energetically accessible and yet suitable for the test of interaction phenomena. The Clementine experiment was designed to perform such tests. There are more such opportunities in the future for soft and hard landings, seismic strength tests, pulverization, and deflection. Such tests could provide much of the data needed for kinetic impact deflection. They would also provide some of the data needed for predictions of deflection by higher energy sources.

Analysis. There is a need to work through a planning cycle, to see how well mission planning, detection concepts, missile configurations, explosives, homing, and control would work under simulated scenarios. An essential part of this would be the development of improved theory for interaction, negation, and mission planning. There have been a number of studies: the Detection Workshop, the Interception Workshop, various international meetings, and at the Air University. However, these have mostly been top-level studies. There has been little work to tie together the pieces of each workshop—let alone those of the different workshops. Road maps exist for detection, interception, and experiments separately, but there is a need to integrate them with road maps for detection, existing capabilities, and missions. The first step toward this integration would be the creation of some group that maintains activity and continuity in these core areas between intermittent meetings.

Summary. NEOs represent a global threat that apparently justifies continuing efforts on search, cataloging, and preparation for defense. There is a wide range of sizes of objects, which produce a variety of effects. The largest objects are globally devastating but their impacts are infrequent; the smaller objects produce effects that are more localized spatially, but they impact more frequently. Each presents a unique and serious hazard.

The most familiar portion of the threat is the large number of inferred but undiscovered NEOs. Most could be found efficiently through modest ground-based searches, which would primarily require an organized and continuing search program. The Air Force has developed the essential assets in telescopes, CCD technology, focal planes, computers, networks, and facilities for such searches. The key to the effectiveness of such a search is an early start and continuity. It needs a principal sponsor with a planning horizon as long as that of the problem. The obvious candidates are NASA and the Air Force, but NASA is at present uninterested or overcommitted to other areas. The Air Force's operational mission is in many ways a better fit to the requirements for this long-term, careful search.

Long-period comets are currently estimated to represent a significant fraction of the threat. They have more stringent detection requirements and require more rapid response. They could require search times measured in days and interceptors maintained in readiness, both of which would favor space basing of key elements. They are clearly a more stressing threat and hence they will require further development of the technologies for detection, computation, and negation.

For most objects, kinetic energy pulverization appears to be the preferred means of deflection. It would be difficult to perform pulverization experiments on the Earth. Fortunately, asteroids frequently pass the Earth on trajectories that are both energetically accessible and suitable for interaction tests. Clementine was designed to perform such tests; there will be more such opportunities in the future. Soft and hard landings, seismic strength tests, pulverization, and deflection tests could provide much of the data needed for kinetic deflection as well as others. It should not be difficult to develop means for the international oversight and/or control of conventional missiles and explosives.

Summary of Recommendations

In Volume 1, *Space Surveillance*, of the study *Space Surveillance, Asteroids and Comets, and Space Debris*, the Committee recommended that the acquisition of Improved GEODSS, with a CCD focal plane array and improved software, be completed and be more fully deployed. The present deployment plan is inadequate. This recommendation in Volume 2, *Asteroids and Comets*, assumes that the recommendation in Volume 1 has been completed.

Separate efforts are appropriate for the various sizes of objects. For large NEOs, ground-based search with large CCD focal planes on existing telescopes is appropriate. That could be done on a shared basis with existing telescopes, although it would be preferable to perform the searches on telescopes made operational from storage. The hardware costs would largely be for mounting of the telescopes and CCD arrays that already exist. Computational support should be possible with 486-class PCs, and operational impact should be modest.

Long-period comets are more stressing. For them it would be appropriate to begin work on space-based telescopes, including infrared focal planes, for rapid search.

In would be appropriate to study means of deflection, particularly kinetic pulverization, in order to define better the requirements for negation. Further useful theoretical work can be done, but experiments are needed that could not be performed on Earth. Experiments should be performed on asteroids that pass the Earth on suitable trajectories. Soft and hard landings, seismic tests, pulverization, and deflection tests should be performed to obtain the data needed for deflection concepts.

It would be appropriate to execute a planning cycle to see how mission planning, detection, homing, deflection concepts, and control work under simulated scenarios. A number of workshops and studies have addressed these issues at the top level, but it is necessary to integrate their outputs into an overall assessment. It would be useful to create a small group to maintain activity and continuity in core areas between meetings; its goal would be the creation of a strawman configuration for detection of large NEOs and LPCs and the kinetic interception of intermediate-size objects. The Air Force is ideally suited for the establishment of such a Center of Excellence.

In summary, in the area of asteroids and comets, the Committee recommends that the Air Force

- Take the lead in detection of NEOs and LPCs
- Mount a ground-based search program for large NEOs with upgraded GEODSS telescopes
- Create a small group of knowledgeable scientists and AFSPC military operators
- Integrate the output of past workshops into the overall assessment
- Remain cognizant of activities within the astronomy community relating to studies and experiments of kinetic pulverization and deflection
- Plan spaceborne sensors program to detect small and intermediate objects and LPCs

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Acknowledgments

The United States Air Force Scientific Advisory Board and the Ad Hoc Committee on Space Surveillance, Debris, and Asteroids and Comets acknowledge the participation of and express thanks and appreciation to Air Force and NASA organizations for their outstanding support throughout this study. Their genuine interest, encouragement, and cooperation made this study possible. We are especially thankful for the participation in the study by representatives of U.S. Air Force Space Command, the Space and Missile Systems Center, Phillips Laboratory, and the National Reconnaissance Office.

Special thanks are also due to the staff of the Air Force Space Command, who facilitated all phases of this study. Thanks also to the staffs of the Air Force Scientific Advisory Board and to ANSER (Analytic Services, Inc.), who assisted in the final outbriefings and preparation of the report. Their dedication and untiring support of our study did not go unnoticed.

F. Robert Naka

For the Committee,
F. Robert Naka, Chairman
1 November 1996

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Table of Contents

List of Figures

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Surveillance for Asteroids and Comets
(Including Defensive Measures)

Gregory H. Canavan, O'Dean Judd, and Antonio Pensa

Introduction. Although the Committee's recommendations cover only the surveillance portion (detection, tracking, and cataloging of asteroids and comets and, perhaps, size and rotation) of planetary defense, this analysis presents the entire subject for the sake of completeness.

Interest in planetary defense was stimulated by the recognition, which has emerged only over the last few decades, that the Earth, like other members of the Solar system, is periodically subjected to devastating impacts from asteroids and comets. The most vivid recent evidence is contained in the images in Fig. 1 of the 20 fragments of comet Shoemaker-Levy 9 approaching and impacting Jupiter. An indication of scale of their effects is that the rings propagating out from the points of impact of even the smallest objects are larger than the Earth. These impacts have the potential for global and catastrophic damage; thus, these images could well illustrate what could happen to the Earth in the next millennia—or the next year.

This note discusses the issues involved in planetary defense. The next section provides a brief background on previous work on the problem. The following one summarizes much of what is known about the threat and likely losses. The section on detection discusses current and advanced options for search, their strengths and weaknesses, and likely costs. The section on interdiction does the same for defensive measures—deriving the optimal combination of detection and interdiction and balancing it against marginal benefits to determine the conditions under which defenses are economically appropriate. Defenses are generally appropriate, given unrestricted choice of concepts. Even with a restricted set of conventional tools, they remain effective against the intermediate objects that produce the most frequent regional catastrophes.

This discussion of detection and interception tools provides the basis for an assessment of theoretical and experimental tools required to support defenses. The detection and interception tools would replace current order of magnitude estimates with improved analyses; the theoretical and experimental tools would build from current telescopes, focal plane arrays, satellite sensors, and flyby experiments to perform inspections of space objects of convenience. There are engineering concerns about launching and controlling such defenses, but the greatest concern is the time scales for their development. People are not protected by technologies in development; defenses must be developed and deployed for utility. In determining the time lines for development, it is important to recognize that different elements of the threat have different time scales, which should shape the overall program balance chosen. To assist that discussion, several strawman programs are generated and discussed. Their resource requirements are estimated and found to be modest compared to the potential losses from this threat, which is now understood well enough for action.

Background. A number of investigations stimulated the recognition that the Earth is frequently subjected to devastating impacts. The first was the recognition that the craters on the Moon shown in Fig. 2 were the result of impacts, not volcanism, which was only confirmed by the space program.[1] The second was the recognition that eroded craters on the Earth such as the Meteor Crater shown in Fig. 3 were also the result of impacts, not volcanism, which was confirmed by extensive exploration that found the numerous, large craters spread about evenly

1 D. Morrison, Chair, "The Spaceguard Survey," NASA report, 25 January 1992.

over the stable geologic formations as shown in Fig. 4. The third and pivotal recognition was that the layer of Iridium found all over the Earth at the Cretaceous-Tertiary boundary 58 million years ago is consistent with the impact of a large metallic asteroid about 8 km across, which is thought to have caused the extinction of about 90 percent of the[2] species then in existence. The location of its impact has apparently since been determined, and physical effects of its impact, such as the effects of tidal waves, found in the fossil record.[3]

Recognizing that this presented a potential hazard, in 1990 Congress called for two workshops. The *Spaceguard Survey* dealt with the requirements for detecting threatening near-Earth objects (NEOs), i.e., asteroids or comets with periods of a decade or less, which would bring them within range of few meter telescopes within a decade or two.[4] The *Near-Earth Object Interception Workshop* dealt with the requirements for intercepting threatening NEOs of any period—NEOs, long-period comets (LPCs) with periods of centuries, or objects from outside the solar system.[5] This differing emphasis was intentional, but it was addressed and partially rectified in a series of meetings at the University of Arizona,[6] the Ettore Majorana,[7] and in Chelyabinsk-70,[8] Convergence was assisted by Department of Defense assessments through the U.S. Air Force Air University *Spacecast 2020*[9] and subsequent technical reports.[10] The recent *Planetary defense Workshop* produced a balanced treatment of detection and interception for objects of various sizes and periods as well as a sharper focus on intermediate sized objects, which are most likely to hit the Earth and for which current defensive tools are best suited.[11] The *Planetary defense Workshop* and the recent United Nations[12] meeting produced new insights into program options and integration and enthusiasm for international cooperation. Their implications are discussed below, but first it is necessary to discuss the technical status of the different technical fields required for NEO detection and interdiction.

The Threat. It is estimated that there are about 2,000 near-Earth objects (NEOs) larger than one kilometer on short-period orbits that can intersect the orbit of the Earth—of which less than 10 percent have been discovered. Figure 5 shows the cumulative number larger than diameter D, which falls roughly as $1/D^2$ from D _ 10 to 1,000 m. The error bars are small at large D, because these objects are quite bright, and most of them are thought to have been found. The error bars are about a factor of 10 at small D, where the objects are dimmer, so the distribution has been sampled incompletely.[13]

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3 L. Alvarez, W. Alvarez, F. Asaro, and H. Michel, "Extraterrestrial cause for the Cretaceous-Tertiary extinction," Science 208: 1095-1108, 1980.

4 D. Morrison, Chair, "The Spaceguard Survey," op cit.

5 G. Canavan, J. Solem, and J. Rather, *Proceedings of the Near-Earth Object Interception Workshop*, Los Alamos National Laboratory report LA-12476-C, February 1993.

6 T. Gehrels, ed., *Hazards due to Comets & Asteroids* (Tucson & London, the University of Arizona Press, 1994)

7 T. von Hoff, ed., Erice International Seminar on Planetary Emergencies, 17th Workshop: The Collision of an Asteroid or comet with the Earth (New York, Springer Verlag, in press).

8 V. Simonenko, Proceedings Planetary defense Workshop (Chelyabinsk-70, in press).

9 *Spacecast 2020* (Montgomery, Air Univ press, 1994).

10 L. Bell, W. Bender, and M. Carey, "Planetary Asteroid Defense Study: Assessing and Responding to the Natural Space Debris Threat," thesis (Montgomery, Air Univ press, 1995).

11 J. Nuckolls, ed., *Proceedings, Planetary defense Workshop* (Livermore: University of California, in press).

12 J. Remo, ed., Near Earth Objects: The United Nations International Conference (Annals of the New York Academy of Sciences, vol. 822, 1997).

13 "The Spaceguard Survey," op cit.

This collection of objects periodically traverses the Earth's orbit. Occasionally one hits it. That gives rise to the cumulative impact intervals (frequencies) shown in Fig. 6 as a function of impact yield _ 1/2 $r(D/2)^3V^2$ for an object of density ρ and velocity V^2, which is numerically about 100 megaton (MT) for a 100 m object. Impactors the size of the 8 km, 10^8 MT K-T asteroid have an impact interval of about 50 million years. Tsunami waves from _ 300 m impactors occur at an intervals of about 10,000 years; thus, there is about one chance in a hundred one will impact during an average person's life span, and perhaps one chance in ten that he will die of it. Damage from air disintegration of stony meteorites, like the one over Tunguska in Siberia in 1908 should occur about every 300 years. NEOs a few meters across hit the atmosphere each month and are detected on warning systems.[14]

The energies on Fig. 6 are enormous. For K-T sized impactors, even the energy scales used for nuclear explosives are a million-fold too small. The release of these energies, largely in the form of molten material and ash ejected back out of the impact point, can have widespread and even global effects. A rough indication of their extent is given by Fig. 7, which shows the transmission of sunlight to the ground as a function of impact energy.[15] Even 10^4 to 10^5 MT, i.e., _ 0.5 to 1 km, impactors could block photosynthesis. Because much of the mass is in sub-micron particles injected into the stratosphere, it could persist for months. Because ejected particles would heat strongly at high altitudes during deceleration, they could produce radiation fluxes large enough to start fires even at the far end of the Earth.[16] Such collisions could cause a major loss of global crop production and consequent loss of animal and human life.

Losses from intermediate sized NEOs with diameters from 0.1 to 1 km are less dramatic, but potentially comparable, because their impacts are more frequent and because they can cause Tsunami, which can generate regional damage from modest energies. Figure 6 shows that there is a probability of about 0.1 per century of a 100 m object colliding with Earth. It is most likely that it would land in the ocean, where it would cause a Tsunami, long wavelength, deep water waves that can propagate to long distances. When a Tsunami reaches a coastline, it “runs up” to significant height, which allows it to propagate far inland, produce significant currents there, and produce damage over a significant fraction of the coastline around the ocean basins into which the NEO falls. For example, Fig. 8 shows the effect of a 600 m NEO falling in the Atlantic Ocean about 500 km off the coast by New York. Within 5 hours the Tsunami has covered Long Island and inundated the coastline 10 to 20 km inland from Boston to Baltimore. While the effects of these intermediate size NEOs are not truly global, they would be regionally devastating due to the concentration of wave energy in coastal regions and harbors, where population and economic value are concentrated.[17]

While the losses from 10 to 100 m NEOs are bounded, they can still be significant if the object strikes or explodes over a densely populated area. Had the Tunguska object exploded over Moscow or New York, its 20 MT energy release would have destroyed either. Even < 10 m NEOs are not without risk. While only the _ 5 percent that are metallic could penetrate to cause

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14 G. Canavan, "Cost and Benefit of Near-Earth Object Detection and Interception," T. Gehrels, ed., *Hazards due to Comets & Asteroids* (Tucson & London, the University of Arizona Press, 1994).

15 O. Toon, K. Zahnle, R. Turco, and C. Covey, "Environmental Perturbations Caused by Impacts," Gehrels, ed., *Hazards due to Comets & Asteroids* (Tucson & London, the University of Arizona Press, 1994).

16 O. Toon, K. Zahnle, and D. Morrison, "Environmental Perturbations Caused by the Impacts of Asteroids and Comets," J. Nuckolls, ed., *Proceedings, Planetary defense Workshop* (Livermore: University of California, in press).

17 J. Hills, "Deepwater Waves and Tsunami Produced by the Impacts of Small Asteroids," J. Nuckolls, ed., Proceedings, Planetary defense Workshop (Livermore: University of California, in press).

direct damage, small NEOs can produce optical signatures that could be mistaken for nuclear explosions, which could have untoward consequences in periods of crisis.

These considerations are summarized by Fig. 9 in terms of the expected fatalities from impacts of various sizes. The top curve gives the average annual fatalities based on a geometric conversion of energy into deaths; the bottom curve gives the average fatalities per event.[18] For both, the ordinate is the impact energy, which can be directly related to the NEO diameter and frequency through Fig. 6. The annual fatalities from small objects are about 10/yr for 30 m to 1 km objects, where they jump up to _ 1 to 5,000/yr. After that they fall, as the collision frequency of larger objects declines. However, the average fatalities per event increase with collision frequency until about 1 km, after which they approach the world population for global effects.

There are two major regions of uncertainty indicated on the figure. The first is the dashed line for intermediate sizes, which indicates a roughly order of magnitude uncertainty in the losses due to Tsunamis from objects of that size. That uncertainty results from the recent recognition of the phenomena and the sensitivity of those losses to geographic effects. The other sensitivity is the uncertainty in the size of impactor required to produce devastating, global effects. The figure shows that uncertainty as covering only the range 1 to 2 km, but the uncertainties discussed above are such that either of these values could well be in error by a factor of two.

Yield, which is the independent variable, can be eliminated between the two plots in Figs. 9 to produce Fig. 10, which shows the global fatality rate from NEO impacts as a function of the number of deaths per event, compared to the rates from transportation accidents and natural disasters. Transportation accidents might involve 100,000 deaths per year, but the number of deaths per accident is at most about 300, i.e., about the capacity of a large airplane. Natural disasters involve a similar total, although the deaths per incident are still bounded by the perhaps 10 million deaths in large floods or earthquakes. NEO impacts have lower fatality rates but deaths per event that are factors of 1,000 to 100 million larger than those phenomenon, which is what sets them apart. They represent a physical example of an event with near zero probability but possibly unbounded loss, which makes their analytical and popular discussion difficult.

Figure 11 shows the physical basis for this difficulty. It plots marine extinctions as a function of time for the last 600 million years.[19] The extinctions are sharp and narrow. The K-T extinction, which was about 40 percent, is clear. The spikes that caused large extinctions are few and widely separated. Some argue for a dominant period of about 26 million years in the extinctions. That would lead to a ratio of peak to average annual fatalities of about 26 million, which is on the order of the ratio seen in Fig. 10. However, that specific period is not essential for the discussion below, which depends primarily on the facts that there have been many extinctions through time and that there has been some success in connecting them with known impacts.

Overall, there is a reasonable bound on the magnitude of the threat based on several centuries of observation. The rough number of objects can be bounded, even if only a modest fraction of them has actually been detected. The situation is better for short period objects than long because they pass by more frequently and brightly; thus, observational results tend to be biased towards them. The statistics and distribution of long period comets known much more poorly. They are seen less frequently, and they may have systematically lower albedos, which has

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[18] D. Morrison, "The Impact Hazard," T. Gehrels, ed., *Hazards due to Comets & Asteroids* (Tucson & London, the University of Arizona Press, 1994).

[19] M. Rampino, "Extraterrestrial Impacts and Mass Extinctions of Life," T. Gehrels, ed., *Hazards due to Comets & Asteroids* (Tucson & London, the University of Arizona Press, 1994).

led to estimates that LPCs are anywhere from 5 to 40 percent of the threat.[20] At the lower end of this range, LPC damage would represent an acceptable uncertainty in the initial survey. At the upper end, they would represent half the threat—and one for which the warning time from conventional surveys would be inadequate—which would vitiate current concepts for NEO searches. However, decadal searches for NEOs would remove many of these uncertainties about the magnitude and character of the LPC component of the threat and hence the need for prompt versus decadal warning about threatening objects.

Detection. As noted above, the search and detection community has largely adopted a goal of identifying, characterizing, and cataloguing potentially threatening NEOs through decadal searches with conventional ground-based telescopes with improved focal planes. The tools for such searches would be derived from those now in use for NEO searches. Figure 12 shows some of those instruments: the modest Palomar film-based Schmidt telescopes and the 0.9 m, f4, charge coupled device (CCD) focal plane Spacewatch University of Arizona Kitt Peak telescope.[21] Over the last two decades this combination has discovered about 100 of the _ 2,000 NEOs larger than 1 km. Palomar was still effective in discovering Shoemaker-Levy 9, although Spacewatch is now making the bulk of the discoveries—about half the world's total. These telescopes have been quite effective, but have intrinsic limitations, which are summarized in the fact that at their current search rate they would take about 100 years to complete the search for the 2,000 NEOs larger that 1 km.

The *Spaceguard Survey* designed a system with a half dozen 2.5 m telescopes with large (16 million detector) CCD focal planes, projected to cost $60–100 M, but it gained little support among funding agencies or sources. After the discovery of Shoemaker-Levy 9, Congress asked for a second review of the search problem, which produced a different answer: that telescope aperture is not the major issue; adequate search for large NEOs could be performed in a few decades by outfitting a few existing telescopes with large CCD focal plane arrays, starting the search soon, and making the modest improvements in existing data handling systems needed to handle the additional data rate and object files expected.[22]

The reason for this change follows from fundamental considerations.[23] A sensor's detection rate R is the product of two factors: its rate of coverage of solid angle, F/t, and its range of detection, _(tA), where F is the sensor's angular field of view, A is its aperture area, and t is its exposure time. Thus, R ~ F_(A/t). For a focal plane with N detectors, F ~ N/A, so R ~ N/_(At), which is maximized by reducing t to the value at which F/t reaches the search rate W at which the sensor just covers the entire fresh sky each month. For smaller t, R ~ W_(tA). Thus, R is maximized for F/t = W, or t ~ F ~ N/A, which gives R ~ _N. At the optimum exposure time, the aperture area A cancels out, and the detection rate is R ~ _N.

Figure 13 shows the results of survey calculations of detection rate as a function of exposure time and telescope diameter for N = 16 million, the number of detectors assumed for the *Spaceguard Survey*. For a 1 m telescope, the detection rate increases with t until about 45 s, where it is a maximum. For larger t, it falls. The 1.25 m curve rises until 25 s and then falls. The 1.5 and 1.75 m curves peak at 20 and 15 s respectively. The 2 m curve falls for all values of t

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20 E. Shoemaker, *Report of the Near-Earth Survey Working Group* (Washington, D.C., NASA, June 1995).

21 "The Spaceguard Survey," op cit.

22 E. Shoemaker, Report of the Near-Earth Survey Working Group, op. cit., pp. 8-18.

23 G. Canavan, "Optimal Detection of Near-Earth Object Threats," J. Remo, ed., *Near Earth Objects: The United Nations International Conference*, op.cit.

shown. The exposure time for maximum detection rate falls as t ~ N/A for the above reasons, and for operation at this exposure time, the detection rate is the same for all aperture sizes.

The reduction of exposure time with increasing aperture is the new feature of the recent *NEO Survey*. Previous emphasis was on long exposures times and large apertures to maximize _(tA) to get the most out of every frame, which overlooked the resulting penalty in the number of frames produced. It is now understood that to perform a good survey as quickly, the best strategy is to cover the whole sky each month to whatever limiting magnitude that results.[24]

A search for objects larger than 1 km could be done in about a decade with a few dedicated 1 to 2 meter telescopes with advanced CCD detector focal planes. They should discover about 70 percent of short period NEOs—perhaps 90 percent with Air Force and international participation. The key element in such a study is good CCD focal plane arrays. Over the last decade, the Air Force has developed, largely through MIT Lincoln Laboratory, the types of CCDs needed.[25] Figure 14 shows a 1960 x 2560 CCD array with high quantum efficiency and uniformity over a broad visible wavelength band, and 8 parallel readout ports, which allow the 5 million detectors to be read out in about 0.3 seconds, and a frame-transfer characteristic that allows the contents of the detectors to be moved quickly to inert cells for subsequent readout, eliminating blur and mechanical shutters. The 80,000 electrons wells permit long integration without saturation by their 10 electron/sec-detector dark current.

Figure 15 shows the limiting magnitude the array can achieve as a function of integration time in dark and bright sky conditions. The curves follow theory, and the values achieved are adequate for NEO search. For example, under dark skies only a few seconds integration would detect a magnitude 20 object, which is roughly equivalent to a 1 km NEO at 1 astronomical unit (AU = 150 million km, the distance from the sun to the Earth). Of course for this application the key parameter is search rate, not integration time. Figure 16 shows the array's search rate as a function of limiting magnitude. The sensor must search about 30,000 sq. deg to cover all of the sky each month. The lower curve shows that a single GEODSS with this array could search the whole sky at magnitude 21, which is adequate for the completeness required of initial surveys.

Figure 17 shows the projected performance of the current GEODSS telescopes with its current camera replaced by one of these 1960 x 2560 CCD arrays.[26] This detailed simulation of search for 120 lunations (10 years) is done for 1 km NEOs with a nominal albedo of 0.15. Initial field results have achieved roughly this discovery level.[27] The GEODSS upgrade completeness rises in 2.5 years to about 75 percent. It then more slowly finds the remaining undetected objects, reaching about 90 percent. The other curves are for other sensors under consideration. LONEOS is a smaller telescope with a wide field of view. It performs about a factor of 2 worse, even with a good CCD array. "Super-Spacewatch II" is a 1.8 m telescope equipped with 16 million CCDs. Although this is a more demanding and expensive instrument than GEODSS, but its performance is not proportionally better. Its completeness increases faster initially, but then rolls off and reaches about 95 percent. Both sensors are simulated near optimum; the difference is that "Super-Spacewatch II" has a factor of 4 more detectors that GEODSS and hence about a factor of 2 higher initial detection rate. It has roughly the characteristics of the sensor designed by the

[24] A. Harris, “Evaluation of CCD Systems for Near-Earth Object Surveys” G. Shoemaker, *Report of the Near-Earth Object Survey Working Group*, op.cit., Appendix III.

[25] T. Pensa, "DoD System for the Detection of Near Earth Objects, Lincoln Laboratory report, 8 September 1995.

[26] A. Harris, "Evaluation of CCD Systems for Near-Earth Object Surveys," op.cit.

[27] G. Stokes, MIT Lincoln Laboratory, October 1997.

Spaceguard Survey, whose sensor should perform similarly, because their 2.5 m aperture would not provide any advantage under optimal conditions.

Figure 18 summarizes a number of the technology options for the NEO search. Spaceguard, Spacewatch, LONEOS, and GEODSS. TOS is a smaller auxiliary to GEODSS. The NASA liquid mirror uses unproved technology to achieve large apertures, although large apertures do not have much impact on this problem. Moreover, the exposure time scales inversely with aperture, and it is not clear that liquid mirrors could be re-pointed on time scales of a second or less. Lawrence Livermore's wide field of view camera pushed field of view to the limit to achieve very high search rates, although its limiting magnitude make it unacceptable for a NEO search instrument. Overall, there are a number of optical configuration options and a number of instruments available, but the key issue appears to be a large, good CCD array on a few fast telescopes designed for search. The key element appears to be the CCD, for which the Lincoln Laboratory ~ $(2K)^2$ array is an appropriate building block.

The discussion above gives a thorough treatment of search for NEOs, because that is what most effort has been concentrated on for the last decade. But there are other threats, which are apparently equally important, that have not been treated as thoroughly—in particular LPCs, which have periods of centuries, so they would probably be seen only on their single pass before impact. They require a different type of search. Rather than scanning the sky annually, it could be necessary to examine the sky each month—or even more frequently, as a LPC detected at 2 AU would only be about 4 months from impact. LPCs could also require a different kind of search. Ground-based searches are confined to within about 60 degrees from opposition to minimize background light and maximize limiting magnitude. Thus, a LPC could come from the direction of the sun and reach the Earth before such a search looked in that direction. There is also some indication that there are low albedo or "stealth" comets. LPCs are not a minor concern; they are thought to constitute 20 to 40 percent of the threat. The problems in searching for them are related to the problems involved in searching for intermediate sized objects that can cause tsunami and enhanced regional damage. They are also dim and hard to see from the Earth, particularly given the limits on what can be seen from Earth through atmospheric turbulence. The need for higher sensitivity and faster response for LPCs and intermediate NEOs both argue for space basing for future space sensors.

Interdiction. The energies involved in asteroid and comet impacts are enormous compared to those that can be generated by man, but fortunately, applying these energies long before impact makes it possible to deflect or disrupt most objects. Figure 19 shows the ability of various interception technologies to negate 10 m to 100 km NEOs given reaction times of months to millennia.[28] The top curve, indicated by dark squares, is the deflection capability of nuclear explosives on interceptors with very high specific impulse. Given a reaction time, T, of decades, they could negate NEOs tens of kilometers across. Diameter increases roughly as D ~ $T^{1/3}$, although the coefficient of proportionality depends on interaction technology and coupling efficiency.

The second curve shows the capability of nuclear explosives on conventional rockets, which merges with the top curve for $T > 10$ years, but falls a factor of 2 to 3 below it for $T < 1$ year. The third curve is for standoff nuclear explosions on conventional rockets and, which waste energy, but improve the symmetry of energy deposition to reduce the danger of fragmenting or spalling the NEO.[29] The fourth curve is for kinetic energy payloads on high specific energy

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[28] G. Canavan, J. Solem, and J. Rather, Proceedings of the Near-Earth Object Interception Workshop, op. cit.

[29] T. Ahrens and A. Harris, "Deflection and fragmentation of near-Earth asteroids," Nature **360**, pp. 429-433.

rockets, which approach the performance of standoff nuclear explosives for $T < 1$ year. The fifth curve is for kinetic energy impact with conventional rockets. The sixth curve is for mass drivers—mechanized conveyor belts that throw surface material into space to produce recoil or other low-thrust, high-efficiency technologies of this type.[30] They have too little thrust to address NEOs with $D > 100$ m with less than a few years warning, but they could deflect _ 2 to 3 km NEOs with a few centuries of warning.

Given a time T to react, the defense must deflect a NEO enough for it to miss the Earth by its radius R_e.[31] Current rockets could deliver a payload mass M_f _ 10 tons of explosive of specific energy density ϕ. For nuclear explosives, ϕ _ 2 MT/ton _ $9x10^{12}$ Joule/kg. For kinetic energy impacts, ϕ _ NEO kinetic energy _ $(30\ \mathrm{km/s})^2/2$, _ $5x10^8$ J/kg, which is _ 100 times that of conventional explosives, but _ 10^{-4} times that of nuclear explosives. The energy release $Mf\phi$ ejects a mass M_e from the NEO of mass m at a velocity v_e, whose recoil gives the NEO a velocity _v _ $M_e v_e/m$. The energy of the ejecta is $Mf\phi$ _ $M_e v_e^2$, so _v _ $Mf\phi/mv_e$. A deflection _vT > R_e requires $D < (kT\phi M_f/R_e\rho v_e)^{1/3}$, where ρ is the density of the NEO and k is a numerical parameter. The dominant scaling is $D \sim (T\phi)^{1/3}$, but the factor of $(k/v_e)^{1/3}$ is also important. For deflection many orbits prior to impact, k _ 3-5, but for unobserved NEOs and LPCs, detection occurs on first approach, much of T is used for fly out, and k _ 0.1.[32] Figure 19 merges these limits in estimating deflection; the effectiveness calculations below use k _ 0.1 for conservatism. The ejecta velocity v_e is poorly known. It varies from _ 100 m/s for optimally buried bursts, _ 1 km/s for surface burst, to _ 10 km/s for standoff bursts.

Over the last decade there have been technical disputes over the relative emphasis to be placed on short warning (_ 1 year) versus long warning (> 10 to 100 years) and over the gap at T _ 1 year between the 100 m capabilities of kinetic energy and the few kilometer capability of nuclear concepts, which has lead to disagreements between advocates of nuclear and nonnuclear concepts. That has been settled temporarily by the recognition that there are about equal short and long term components to the threat. The debate has also been reduced by developments that greatly extend the capabilities on nonnuclear concepts. Figure 20 shows a concept that uses an array of small, dense spheres, precisely deployed by a light, rigid array, to pulverize few hundred meter NEOs into smaller pieces that do not survive entry into the Earth's atmosphere or produce enough blast for wide-scale damage.[33] The energy and mass required to pulverize with strings of objects of low mass and high ballistic coefficient is modest. And letting the atmosphere provide the final layer of the defense removes the need to deflect the debris over large angles. The net energetics are intermediate between those of bulk kinetic and nuclear concepts. This could evolve into a nonnuclear concept that could address kilometer sized objects. Thus, kinetic energy could be adequate for a larger class of object than previously thought, including the most bothersome intermediate objects that cause Tsunami.[34]

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30 H. Melosh, I. Nemchinov, and Y. Zetzer, "Non-Nuclear Strategies for Deflecting Comets and Asteroids, T. Gehrels, ed., *Hazards due to Comets & Asteroids* (Tucson & London, the University of Arizona Press, 1994).

31 G. Canavan, "Cost and Benefit of Near-Earth Object Detection and Interception," op. cit.

32 T. Ahrens and A. Harris, "Deflection and Fragmentation of Near-Earth Objects," op. cit.

33 L. Wood, R. Hyde, M. Ishikawa, and E. Teller, "Cosmic Bombardment V: Threat Object-Dispersing Approaches to Active Planetary defense," Lawrence Livermore National Laboratory report, 22 May 1995.

34 L. Wood, M. Ishikawa, Rod Hyde, and E. Teller, "Cosmic Bombardment V: Threat Object-Dispersing Approaches to Active Planetary defense," J. Nuckolls, ed., *Proceedings, Planetary defense Workshop* (Livermore: University of California, in press).

Figure 21 shows the cumulative damage from impacts of objects up to a given diameter.[35] It does not just assign dollar values to the fatality rates of Figs. 9 and 10; it estimates economic losses from the expected physical losses of Fig. 6. There is a small contribution in the 5 to 50 m region from infrequent penetrations by small metallic asteroids and in the 50 to 200 m region from stony NEO airbursts. The region from 200 m to 2 km is dominated by Tsunamis; that over 2 km by global fires and loss of production. The first two regions are obvious and well documented. The last two are less familiar and deserve some discussion. The 200 m to 2 km region is interesting in that it is doubly covered by two hazards. According to Fig. 6, in that region the cumulative collision frequency $f \sim 1/D^2$, so the differential collision frequency is $f \sim 1/D^3$. An object of diameter D has energy $\sim D^3$, so it produces a lethal pressure p out to radius R determined by $(D/R)^3 \sim p$. Thus, the radius of destruction scales as $R \sim D$, and the area of destruction scales as $R^2 \sim D^2$. The monetary loss is roughly proportional to area, so it is also proportional to D^2. Multiplying D^2 by df/dD and integrating gives _ dD $D^{-3}D^2$ = log (2 km/ 0.2 km). Thus, each octave in the interval gives an equal contribution to the loss. The resulting sum is a logarithm, which is insensitive to details of the integrand or the endpoint. That means all of the intervals in Fig. 6 are of equal concern, including the 100 m impacts of millennial frequency.

However, the damage from impacts on land is only about 10 percent of the loss shown on Fig. 21. There is a larger contribution from Tsunamis, which by coincidence scales the same way, but have a larger value. For Tsunamis the principal input the damage estimate is the result from nuclear tests that the wave produced by ocean explosions or impacts scales as the square root of the yield and inversely with the range r from the detonation, or $h \sim D^{3/2}/r$. There is about an order of magnitude amplification of the wave at the shoreline, after which it runs inland a distance $\sim h^{4/3} \sim (D^{3/2}/r)^{4/3} \sim D^2/r^{4/3}$. Thus, the damage again scales as D^2, and suitable averaging over ocean basins again gives a logarithmic result, but with a coefficient about an order of magnitude larger than that for land impacts. Thus, coincidentally land and ocean impacts give similar contributions, which are an order of magnitude larger than those from small impactors and within an order of magnitude of those from large impactors.

The losses from large NEOs are rough bounds derived from the arguments above that NEOs larger than about 2 km could cause global damage. The Earth's gross product is about G _ \$20T/yr. If the damage from a several kilometer NEO required a recovery time of T_{rec} _ 20 years, the integrated loss would be about $T_{rec}G$ _ \$400T—which is also about the amount that would have to be spent on civil defense for survival through this period to "insure" against large NEOs. There is considerable uncertainty in the data, but it appears that the impact frequency for D > 2 km is $f \sim 1/D^3$; thus, the expected losses from NEOs with diameters between 2 km and D are _ $GT_{rec}[f(2) - f(D)]$ a $(1/2^3 - 1/D^3)$. The losses reach their maximum value of $GT_{rec}f(2)$ by D _ 3. The expected annual loss from NEOs with 2 < D < 3 km is _ \$400T x 10^{-5} km^3/yr $(1/2^3 - 1/3^3)$ _ \$300M/yr, which is roughly the increment between the "sub-global" 2 km and "global" 3 km losses shown in Fig. 21.

These estimates are uncertain. The collision frequency of large NEOs is uncertain by a factor of two. The time persistence of the destruction is probably uncertain by an order of magnitude. The expected loss is uncertain by several orders of magnitude. And the loss of production used to denominate losses above is only a surrogate for value losses that are more difficult to quantify. Moreover, these losses are sensitive to the time for preparation available before impact. If a NEO of this size hit without warning or preparation, the devastation could be global and total. If there was adequate warning and preparation, the above estimate of losses should be reasonable. If there were adequate defenses, losses could be minimal. The first case

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[35] G. Canavan, "Cost and Benefit of Near-Earth Object Detection and Interception," op. cit.

corresponds to living in an uninsured house; the second to building a spare house as insurance; the third to protecting the original house. The first is foolish. Whether the second or third is preferred depends on the costs of the defenses relative to the losses estimated above.

The decision to deploy planetary defenses should be made on the basis of whether they are cost-effective—specifically whether the marginal cost of incremental defenses is less than the marginal reduction of damage by those incremental defenses. The latter is straightforward. Figure 21 gives the losses from objects up to a given size. Differentiating that curve gives the marginal benefit of defending against impactors of that size, as shown in the down-sloping curves in Fig. 22, in which the curves for very small, small, intermediate, and large impactors is clear. The upwardly-sloping curves are those for the marginal costs of defenses against objects of different sizes. The bottom curve is for nominal costs; the curve above it for 10 times nominal defensive costs; and the top curve for 100 times nominal costs. These curves are more complicated to derive. Defensive costs have two main components: the cost of detection of the object and the cost of intercepting it. The cost of interception was discussed in the section on Interdiction, which showed the payload mass required to intercept an object of diameter D scales as $M_f = BD^3/T\phi$, where B is a constant. This shows the payload mass increases rapidly with object mass and falls with warning time and interceptor specific energy. The scaling on 1/T means distant detection can greatly reduce interceptor requirements—and cost.

The cost of detection can be illustrated with the scaling of space based telescopes, which illustrates the main points and produces results that are actually general.[36] The signal required to produce signal to noise SNR is S ~ SNR_(B_{sp}/t), where B_{sp} is the photon noise background, a constant for a diffraction limited space sensor. For optimal search, t a N/A, so S a SNR_$(B_{sp}N/A)$. The scattered sunlight received from an object of diameter D at range r is ~ $AD^2/r^2(1 + r)^2$. Equating the two gives NA ~ $[r(1 + r)/D]^4$. N and A can be varied independently to maximize performance, and the cost of each is proportional to the total cost, so detection costs scale as $[r(1 + r)/D]^2 = H[vT(1 + vT)/D]^2$, where H is a constant that can be estimated from known systems and the last expression substitutes warning time for radius.

Interception costs scale as 1/T and detection costs as T^2 or T^4, so their sum has a minimum at some intermediate time t_{opt}, which can be found by differentiation. Substituting t_{opt} back into the combined cost for detection at r < 1 AU (objects smaller than _ 1 km) gives an optimal cost C_{opt} ~ $(D^2B/\_H\phi)^{2/3}$, which scales on $(B/\phi)^{2/3}$, weakly on A, and strongly on $D^{4/3}$. Figure 22 shows the marginal costs—the derivative of the optimal cumulative cost curves with respect to D—which scale as $D^{1/3}$ from 10 m to about 1.5 km, which is the transition to detection for r > 1 AU where detection costs increase more sharply. The upward sloping curves on Fig. 22 give the marginal costs and benefits of planetary defenses against objects of various sizes for various estimates of defense costs, i.e., of the parameters A and B. Cost effectiveness at the margin means that defenses are effective up to those diameters where the marginal benefit curves fall to the marginal cost curves. For nominal costs, that occurs for objects with diameters of about 8 km, i.e., the size of the K-T impactor. For 10 times nominal costs, defenses are still cost effective for very small, small, and intermediate objects, but the crossing between the cost and benefit curves for large objects shifts to about 4 km. The 100 times nominal costs would make defenses ineffective for small, intermediate, and large objects.

The bottom curve for nominal costs is based on the parametric costs for conventional rockets and nuclear explosives. As shown above, the optimal costs for defense scale as $1/\phi^{2/3}$, so the nominal costs for kinetic energy are a factor of $1/(10^{-4})^{2/3}$ _ 480 higher, which are too

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[36] G. Canavan, LLNL talk sensors G. Canavan, "Cost and Benefits of Near-Earth Object Defense," J. Nuckolls, ed., *Proceedings, Planetary defense Workshop* (Livermore: University of California, in press).

expensive for effectiveness. However, pulverization above does not require deflection by a distance as large as the radius of the Earth. Deflections of a hundred kilometers might suffice to eliminate coherence between separate objects, in which case much smaller deflections and energies might suffice. The deflection velocity is the ratio of the required displacement to the warning time and is also proportional to the payload mass required. The cost of deflection is proportional to mass through the coefficient B; thus, a decrease in the displacement required by a factor of 100 km / 6,400 km _ 0.01 is equivalent to a 100-fold decrease in B. The cost of the optimized defense scales as $(B/\phi)^{2/3}$, so the cost of a pulverizing kinetic energy defense should only be greater than that of a nuclear defense by a factor of _ $(0.01/10^{-4})^{2/3}$ _ 20, which would make pulverization cost effective for very small, small, and intermediate objects and large objects up to 4 km. In short, pulverization would appear to make nonnuclear defenses effective for most objects of concern and most particularly for the intermediate sized objects of immediate concern.

The discussion above has addressed the cost effectiveness of objects detected on final approach. The analysis differs slightly for objects detected long before impact, which are the subject of most search studies.[37] The essential input to such a study is the result derived above that the search rate for an optimized ground-based sensor scales as R ~ _N. Thus, the losses during a search of duration T_s are $L = GT_{rec}f(1 - e^{-RTs})/R$, and the benefits of the search are $U = GTf[T_s - (1 - e^{-RTs})/R]$, whose derivative at large N is $dU/dN \sim 1/N^{3/2}$. If the sensor cost is proportional to the number of detectors, the marginal cost for increasing N is a constant, and the optimal number of detectors scales as $N \sim (GT_{rec}f)^{2/3}$. Figure 23 shows the marginal costs and benefits for typical cost parameters and various values of Tsearch. For a nominal cost of $100M for a search system with 4 million detectors, for a 5 year search the optimal sensor size would be about 10 million detectors; for a > 10 year search it would be about twice that. For such a system the savings would be on the order of $2.5B, so the net benefits of warning could be very large, if warning was adequate to avoid most losses.

Theory. The sections above have given a first order account of the main features of conventional and unconventional detection of objects of various sizes and nuclear and nonnuclear defenses against them on long and short time scales. There is a rough body of analysis that covers each, but the analysis is thin, and there are places that are hardly covered at all. For some key areas such as kinetic pulverization, only ad hoc, piecemeal theories exist, and there is an inadequate understanding of the objects themselves. Heretofore, analyses have treated comets as rubble piles and asteroids as highly competent rock. Recent observations—including Shoemaker-Levy 9's impact on Jupiter, are consistent with the impact of an object of zero strength, which has made it necessary to reconsider those assumptions. The strength of these objects is a fundamental consideration in deflection and pulverization calculations. For the former, it determines how much energy can be coupled into an object before it is converted into a spray of fragments whose overall velocity may have been changed little. For the latter it could reduce the energy requirements for pulverization, making that mechanism even more effective. There is clearly more work needed on interaction, coupling, and mitigation of unexpected effects for the full range of alternative defensive concepts.

Experiments. There is a clear set of steps leading from theory and laboratory experiments to space flyby and probe experiments, which could establish the basis for an impact mitigation options matrix. The steps lead through continuing up- and down-looking observations to actual space experiments. Figure 24 illustrates ground-based, wide-field telescopes executing

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[37] G. Canavan, LLNL talk sensors G. Canavan, "Cost and Benefits of Near-Earth Object Defense," J. Nuckolls, ed., *Proceedings, Planetary defense Workshop* (Livermore: University of California, in press).

systematic sky surveys, which have been the most productive part of the asteroid search effort to date. Although, as indicated in Fig. 12, searches have to date been executed primarily by university and academic institutions, those searches are now reaching the level of complexity and cost in terms of telescopes, focal plane arrays, data processing, cataloguing, and filtering defense data to warrant direct federal involvement. There are adequate telescopic assets in the form of sensors such as the existing GEODSS telescopes, which when fitted with advanced detector arrays of the type shown in Fig. 14 would be well suited to both decadal searches for NEOs and rapid searches for intermediate objects on final approach. Such systems already have appropriate data processing and archiving to integrate NEO searches into normal operations. Some exploratory work has already been done. Figure 25 shows the Lincoln Laboratory's detection of Asteroid 114 Kassandra near Omega. These defensive systems apparently have the capability to perform effective searches to the resolution desired without interfering with normal duties.

Down-looking defensive sensors have also added a great deal to knowledge of NEO structure. Figure 26 shows signatures of NEO impacts as seen by warning satellites. These bright, well-resolved signatures provide significant information on the size, composition, and strength of objects as they decelerate and break up in the atmosphere. However, those breakups can at times produce multiple maxima that could be mistaken for nuclear explosions, which could be confusing in times of crisis. Thus, to understand both the physical information contained in those signatures and how to avoid false interpretations, down-looking sensors are a useful area for further study. There is adequate material. Figure 27 shows the geographic distribution of impacts, which cover the globe uniformly, and the temporal distribution of their arrival, which shows that they are not just local debris. The combination of down- and up-looking sensors appears particularly capable.

Telescopic observations have been valuable in estimating sizes and giving some classification of coarse composition, and radar observations have given reflectivity information and reconstructions of a limited number of objects. However, neither gives much insight into their mechanical strength and internal composition, which are the primary parameters needed to estimate how hard it would be to deflect them without reducing them to unmanageable rubble. That requires closer inspection. Ultimately, it will be necessary to visit a selection of these objects and determine their properties first hand.

Such inspections could be performed affordably with the small, inexpensive spacecraft developed by the DoD for missile defenses. Figure 28 shows the sensor portion of the Clementine satellite used to remap the Moon at high resolution.[38] By reducing size, it was possible to package a half dozen high quality instruments on a bus weighing only a few tens of kilograms. Such packages could perform carefully diagnosed space interaction experiments. They could observe the surface, composition, shape, and motion in great detail in flyby experiments at close range, and they could use soft and hard landings to determine objects' strength and response to impulsive loading. These experiments could culminate in kinetic energy impact probe experiments and precise measurements of orbit change by ground- and space-based sensors. That would both study the impulse transferred by kinetic energy impact and simulate the interaction of higher energy sources. Throughout, they could use technology consistent with that for subsequent defensive applications, which would leave a residual intercept capability. A key element would be international execution of experiments, interpretation of data, and integration.

Operational considerations. An operational defense involves a large number of functions. The sections above discussed the principal scientific functions—surveillance, detection, tracking, characterization, and catalog maintenance needed to eliminate false alerts—

[38] L. Wood, private communication.

but there are a number of other functions needed to execute a defensive mission, which include warning and assessment to provide valid alerts of known confidence levels, mitigation of prompt effects, and intercept initiation and control. And of course, for insurance in case active measures fail, it is necessary to have civil defense and crisis response to minimize losses. The scientific functions have been discussed above, and the civil defense functions are global extensions of familiar mechanisms. There are however, a few points that require discussion.

The first are technical. Intercept initiation and control require the prompt launch of an interceptor and its control throughout insertion, transition to deep space, fly out, and homing. Each is a difficult operation that would tax current capabilities. Their combination has been studied in some detail.[39] In brief, it appears that the sensor systems described above should provide adequate warning for decadal threats, though only marginal warning for prompt threats. However, it should be possible to launch the interceptors reliably. There are also adequate combinations of boosters, small busses, and explosives to support a number of concepts for each type of object. There is also an adequate supply of homing sensors from missile defense for the acquisition, homing, and impact with adequate growth capability in closing velocity against large objects on unperturbed ballistic trajectories. There is also a large backlog of advanced technologies for improving these capabilities, although the rate of progress from advanced to available technology seems quite slow.

This leads to an important point. Defenses are not provided by technologies in development. They must be deployed to have any positive benefit. For some threats, it might be possible to wait until they are identified concretely for defenses to be developed. For others, such as LPCs on final approach, it would not be. It is useful to characterize various elements of the threat according to the amount of warning time they would give the defense.[40] *Long Warning* indicates the decades to centuries that might be available for the negation of NEOs detected many orbits prior to impact. *Short Warning* indicates the months to years that might be available for comets or undetected NEOs. *Very Short Warning* is the days to weeks of warning that might be available for intermediate objects.

Long Warning times of decades to centuries result from the detection of short-period (4-10 years) NEOs many orbits prior to impact. For example, detection of a 4-year period NEO 5 orbits prior to impact might give about 100 years for reaction, which would allow considerable development of defensive measures prior to their deployment. Long Warning permits efficient search with conventional telescopes with large CCD arrays. Current and planned ground-based telescopes should be able to provide the search rates required to survey 90 percent of large NEOs in one to two decades. Space-based sensors should be available for their augmentation, and even more advanced technology should become available for upgrade during the few decade search interval, if needed. Long Warning also makes it possible to take full advantage of efficient interception tools such as optimal trajectories, deflection at perigee, and low-thrust, high specific energy engines and provide enough leverage to take advantage of efficient deflection concepts such as ion thrusters, solar energy, mass drivers, kinetic energy, etc. The highly efficient performance of each of these interception and deflection approaches with Long Warning has been made plausible through analytic studies, although they need more detailed systems and engineering studies.

The distinguishing feature of Long Warning is that the most important element of an effective response to this class of NEOs is the prompt initiation of search in order to minimize

[39] G. Canavan, J. Solem, and J. Rather, Proceedings of the Near-Earth Object Interception Workshop, op. cit.

[40] G. Canavan, "Integration Panel Summary Report," Los Alamos report LA-UR-95-1937, 1 June 1995.

overall benefits, which can be performed by modest and inexpensive telescopic searches. If that search found most of the currently unknown short-period NEOs in a few decades and did not find any that represented an immediate threat to the Earth, it might not be necessary to develop means for interception at all. Thus, there appears to be the potential for eliminating risks amounting to a few tens of $B by performing a search which would cost a few $10M. This roughly 100-fold leverage is so great that it would appear cost effective to develop the means for interception even if they were never needed for NEOs in this class. That is particularly so in if they were so constructed that they were applicable to the other classes of objects, which do require defenses.

Short Warning such as the _ 4 months that would result from the detection of a LPC or undetected NEO at _ 2 AU on final approach admit of far fewer search and intercept concepts. Adequate warning against large LPCs requires search to magnitudes of 23 to 24, which requires either massive ground-based telescopes or space-based sensors, either of which could cost _ $100M. Reducing the adverse leverage from the object's high velocity would require the high specific impulse and thrust of nuclear fuels, which would be difficult and expensive to develop. Reducing the penalty from the object's great mass would require payloads with very high specific energy, although that would not require expense or development; current versions could suffice. Short Warning requires that defenses be in place when the threat is detected; there would not be time to develop them later. However, if defenses were developed for Short Warning, which appears justified on the basis of the losses from comets alone, they would also be available to Long Warning threats, although the converse is not true.

Very Short Warning also requires ready defenses. It needs very fast, wide area search, independent of solar viewing and weather restrictions, for which space basing provides further leverage. For intermediate-size objects, the sensor requirements are not excessive. A _ 1 m sensor, which could detect a 1 km object at _ 1 AU, could also detect a _ 0.1 km object at _ 0.1 AU, which would give a warning of about a week. It would not be possible to deflect such an object totally away from the Earth with that little warning, but it should be possible to put a kinetic energy payload in its path that could disrupt it sufficiently such that the Earth's atmosphere could deal with the residue. For intermediate objects, this kinetic energy defense could be developed through a modest number of experiments, whose most stressing elements would be the development of quick-response rockets, homing technology, and control technologies. Should a target require more energy than kinetic energy could provide, it should be possible to substitute more energetic means into the interceptor without invalidating these experimental results or requiring revalidation of the defenses.

Program considerations. To provide interim answers to questions about when it might be possible to develop defenses against various threats, it is useful to sketch out strawman programs that would meet the timelines for the various objects discussed above. A strawman program for Long Warning is:

Long	00	05	10	15	20
search			50 percent	90 percent	
intercept	current		maintain		
improved	study		develop		

The three main elements of the program are search, intercept, and improved technology. Adequate search could be performed by one or two, few-meter ground-based telescopes with upgraded CCD focal planes, which should achieve about 50 percent completeness against short-period NEOs in a decade and 90 percent in two. The completion of this search is the pacing item for defenses against objects in this class, and the pacing item for completing the search is getting it started quickly with modest telescopes with good focal planes.

Near-term interceptors would be slight modifications of current deep-space probes. The improved technology development indicated would involve modified boosters, efficient upper stages, improved deflection means, and more accurate control technologies. After the initial study, key concepts and components could be developed further, although it would not be necessary to integrate them before the results of the telescopic search was known, which is why the intercept programs are paced to the search program.

For Short Warning, a few additional program elements must be added to address the deeper, faster search needed and the need for ready defenses and the experiments required to support them. The resulting strawman program is:

Short .... 00 .... 05 .... 10 .... 15 20
search........... study.................. ....develop.......... operate ....
search space.. study.................. ....develop................ ....deploy
interact exp .... plan .... execute......... complete................
intercept tech . plan .... test ....residual

The key elements are the need to develop much more powerful ground- or space-based sensors. The former would build on, but represent a major extension of, the technology envisioned for near-term, decades long telescopic searches. The latter would involve the development of new search technologies with performance significantly beyond that currently available for observation from space. For that reason, the space-based sensors could take longer to develop and be available later somewhat later.

Interaction experiments are paced by the space experiments needed on flyby observables, probes, and kinetic energy deflection. Fortunately, they could be performed with existing or surplus assets. The interception technology experiments could largely be performed as the control mechanisms for those interaction experiments, which would speed the execution and lower the cost of each. These interaction experiments with kinetic energy should also adequately simulate the coupling of very high energy explosives as well. The value of both sets of experiments would be enhanced in both their execution and interpretation if they could be performed with international cooperation as full as possible.

For Very Short Warning, the strawman program is similar, although some of the lines have slightly different interpretations:

Very Short ..... 00 .... 05 .... 10 .... 15 20
search ground plan .... modify................ ....
search space ... plan .... modify................ ....
interact exp .... plan ....complete............... ....

intercept tech .plan ....residual................ ....

The distinction is that this program addresses intermediate size NEOs; hence, it can use components that can work with less response time because complete deflection or fragmentation is not required. The Earth's atmosphere can provide some of the defense. The requirement is that the object be broken up and dispersed enough so that the fragments would not survive in enough size and number to coherently produce a tsunami. For such a system ground-based sensors derived from those for Long Warning search (e.g., more 1 m telescopes with focal planes that would support an order of magnitude greater search rates) could provide adequate warning, although modest space-based sensors could be developed rapidly to extend search closer to the sun and as a backup to terrestrial weather interruption of ground-based search. These simplifications should enable these modest sensors to be developed significantly more quickly and with less expense than those for Short Warning.

Interaction experiments would be similar to those for Short Warning, they would just be done faster, which should not involve technological issues, just modest augmentation of resources. Similarly, the intercept technology could easily be accelerated to complete all of the experiments required to reach the desired residual intercept capability sooner. The net effect is about a five year acceleration of the strawman program for Very Short Warning compared to that for Long or Short Warning, which largely stems from the reduced levels of performance required from search sensors, the simplicity of the negation concepts, and the direct applicability of existing missile and intercept technology.

These strawman programs appear technically reasonable, but it is appropriate to provide some indication of the resources assumed in constructing them. For *Long Warning*, the three main elements of the program would be started in parallel. The search program would involve the several, few-meter telescope program outlined above. Achieving the 50 and 90 percent completeness levels shown is estimated to cost about $5M/yr, although the times for reaching the latter could be accelerated about a factor of two by funding of about twice that amount. The intercept program is a modest one of studying and modifying current rockets for more accurate deep-space rendezvous, which would also cost about $5M/yr.

The element for the development of improved technology for boosters, deflection, and control would shift at the five year point from studies to technology demonstrations and experiments, which would cost somewhat over $5M/yr. It would not be useful to accelerate the intercept and technology programs without accelerating the search program as well. Together the three elements as shown would require about $15M/yr, or a total of _ $225M over the 15 year development program shown. The $15M/yr would be about 3 percent of the expected annual losses from NEOs in this class. Accelerating the program to $30M/yr would not improve its cost-effectiveness during the development phase, but would speed the date at which actual defenses would be provided.

For *Short Warning*, there is an essential requirement to develop much more powerful sensors. Since it is not clear whether ground- or space-based sensors would be required, it would be appropriate to start their development in parallel. For ground-based sensors, that could probably be done as an extension of current telescopic approaches for an additional _ $5M/yr, for a total of _ $10M/yr for ground-based sensors. The conventional wisdom is that space-based sensors would cost an additional _ $20M/yr and take another 5 to 10 years for development. Although current concepts challenge that wisdom, those numbers are used below.

The series of progressive space interaction experiments needed could be performed in about 15 years for about $30M/yr by building on the technology demonstrated in Clementine, which would make their results available at about the same time as the sensor developments and the integration experiments that would use them. Those interaction experiments could also be used to test interception technology for an additional _ $20M/yr. That would give a total requirement for the Short Warning program of _ $80M/yr, or $1.2B over 15 years, which is $1.2B/ ($0.5B/yr x 15 yr) = 16 percent of the losses expected from objects in this class over that interval. It should be noted that while the Long Warning strawman plan would produce only a survey at the end of 15 years, the Short Warning strawman would produce a significant residual defensive capability. However, because of the long development times for the sensors for Short warning, it is more difficult to effectively accelerate its timelines.

For *Very Short Warning*, it is necessary to upgrade and deploy more ground-based telescopes, although they could be modest sensors at the GEODSS level, which are readily available, which would cost about $10M/yr. It would also be appropriate to augment them by deploying modest space telescopes for greater coverage at angles closer to the sun, which might require an additional $20M/yr. Since those two steps would accelerate the completion of the sensor elements to about a decade, it would be appropriate to do the same with the interaction and interception technology experiments, although that should not add appreciably to their costs, which would remain at _ $30M/yr and $20M/yr, respectively. This acceleration would produce a defensive capability against intermediate objects about five years earlier than the other programs for an investment of about $80M/yr x 10 years _ $800M, which is about half the expected losses from intermediate objects during that interval. Because of the development times for sensors and execution times for interaction and integration experiments, it would be difficult to further accelerate these timelines. However, these defenses, once developed would be available to support the detections from other warning times.

Overall, Long Warning would only lead to an assessment after 15-20 years, not a defense. Since it does not lead to a defense, it can be argued that search only would be about as effective, as has been assumed in earlier studies of objects in this class. A program for Long Warning is the lowest cost option, and does address a significant part of the threat, so it should not be excluded. A program for Short Warning addresses the portion of the threat that cannot be treated by telescopic searches of extended duration. It handles the other global part of the threat, but because of the sensor development required, it does so on a long time scale and at considerable expense. A program for Very Short Warning would lead to both search and defense for regionally threatening intermediate objects. It would also lead to a defensive capability and backup for objects from longer warning searches. The cost for all three of _ $175M/yr would be cost effective relative to the threat, but would constitute a significant increment. Thus, it is useful to examine various combinations that could be undertaken at lower annual costs.

In evaluating combinations, it has been conventional to first examine expanding the program for extended search to Long Warning and second to extend it to include Short Warning defenses. Since Very Short Warning defenses against intermediate objects are the newest concern, there is a tendency to treat them as third place. Given the arguments and estimates above, it is not clear that is the appropriate path. Extending to defense with Long Warning is clearly appropriate, because it is a modest extension and that is highly cost effective. But based on the timelines and technology developments outlined above, it would appear that the second step should be defenses against Very Short Warning intermediate sized objects, because that would provide an early capability against the threats that are likely to be encountered first.

Executing the Very Short Warning program would start towards the improved ground- and space-based sensors needed for other warning regimes, the limited interaction experiments needed for non-threatening technologies, and adequate demonstration of intercept technology integration. Executing the programs for Long and Very Short Warning defenses together would only cost about $100M/yr, but it would address both the large objects that can be detected by extended search and the intermediate objects that can be negated on final approach. If Short Warning defense was added later, it could build on the technology developments and experiments performed by the two earlier programs to produce defenses using longer ranges and greater specific energies to negate larger objects.

Priority activities include studies, interaction experiments, integration experiments, and operations. Studies should cover the detailed search, interception, and deflection technologies discussed above and define their integrated performance at the level required to estimate their performance and cost. To support this, there is an immediate need for systems concept definition, mission analyses, system engineering, and technology studies to guide the follow on phases. Given adequate definition of the candidate defensive system, various professional societies could make important contributions and add credibility to these assessments. An important part of this activity would be the proper assessment and documentation of expected losses, e.g., a handbook of the likely effects of and expected losses from tsunami in various basins. These studies could be an important measure in maintaining communication among active workers and in initiate educational efforts in the field of NEO defenses. This study phase should cover a few years and could be executed at a funding level of a few $M/yr.

The experiments phase should involve both laboratory and then space experiments. The laboratory experiments should be as thorough as possible, and should test the micro- and macro-mechanical properties of a wide range of candidate objects. These experiments could profitably overlap the study phase somewhat, as they would thereby provide useful focus and guidance. The space activities, which should progress through flyby, probe, and deflection experiments supported and diagnosed by ground-based sensors, could build on the successful Clementine technology, approach, and cost structure. If so, the experimental programs could probably be executed for a few $10M/yr. These space experiments should be thorough, as any defenses should be based on knowledge of the NEO's characteristics.

International contributions could be very useful in adding unique diagnostic techniques and in integrating U.S. strength in search, homing, and impact technologies with complementary international strengths in rockets, timely response systems, and space science. These space capabilities, although pivotal, are now fragile and possibly transient. However, it might be possible to augment them through the use of deactivated strategic missiles for experiments, if the value of that option could be agreed to quickly. These experiments would also provide a mechanism for practicing through joint field activities the coordination of command and control procedures that could later be useful for actual defenses. The value of these activities would be enhanced if the data could be transferred to an international center for data analysis, which could in time evolve into a joint warning center for NEO hazards.

Integration experiments could overlap the space interaction experiments somewhat, as they would be intended to test the integrated performance of the search, interaction, and command elements, all of which would be needed for precise, properly diagnosed interaction experiments. If this synergism was exploited properly, the integration experiments could be performed for an amount comparable to that for the interaction experiments. Subsequent to these development programs, the residual intercept capability could be maintained for about $100M/yr, with the greater amount resulting from continuous operations and higher reliability, whose timelines and costs would be paced accordingly.

Summary. There is a significant threat from intermediate and large objects; however, it is possible to both detect and interdict them. The theoretical and experimental work needed to validate those predictions is now well defined. There is an adequate set of tools for negating each class of threat objects, which vary depending whether the object is detected with Long, Short, or Very Short Warning. For Long Warning there are many options for search and negation based on current technology. As warning is reduced, so are the options. However, they remain adequate to span significant and important parts of the threat with non-threatening technologies, which have the capacity for growth to address much larger objects.

These predictions will have to be confirmed through further studies, but if their results are positive, it should be possible to define affordable laboratory and space experiments to demonstrate the effectiveness and affordability of these defenses. These experiments could be performed within one to two decades and could result in a residual capacity for the interception of an important class of intermediate size objects and in the development of interceptor and control capabilities that could support negation technologies of any required energy.

Programs for defensive alternatives depend most strongly on the sizes of threat objects and the warning time they permit. Long Warning programs for objects observed many orbits prior to impact have been studied extensively, but do not lead to actual defenses and address only a portion of the threat. Short Warning programs remedy those deficiencies, but require significant technology development, cost, and delay. Very Short Warning programs could produce early defenses against the most likely threats, which they could address with non threatening technologies with growth potential to objects of all sizes. Executing the Very Short Warning program would move towards the improved ground- and space-based sensors needed for other warning regimes, the limited interaction experiments needed for non-threatening technologies, and adequate demonstration of intercept technology integration. Executing the Long and Very Short Warning programs together would cost about $100M/yr, but would address both the large objects that can be detected by extended search and the intermediate objects that can be negated on final approach with defenses that could build on these technology developments and experiments to produce defenses against larger objects.

It is clear that the threat from both intermediate and large space objects exists. It appears that adequate technology to search for and intercept them exists or can be built on current technology. It also appears that straightforward experiments could be performed to test these technologies and find out the performance and effectiveness of these defenses relative to the expected losses from the impact of these objects. On the basis of preliminary studies, it appears that a combination of search and defenses is more effective than detection alone. A modest combination of studies, laboratory and space experiments, and intercept technology developments, which could be performed openly with scientific and international cooperation, could refine those estimates and objectively assess the ultimate effectiveness and affordability of the full range of defenses needed. There are excellent opportunities for international collaboration in this assessment, and in the testing and deployment of these capabilities, if the formulation and initiation of a responsive program is addressed soon.

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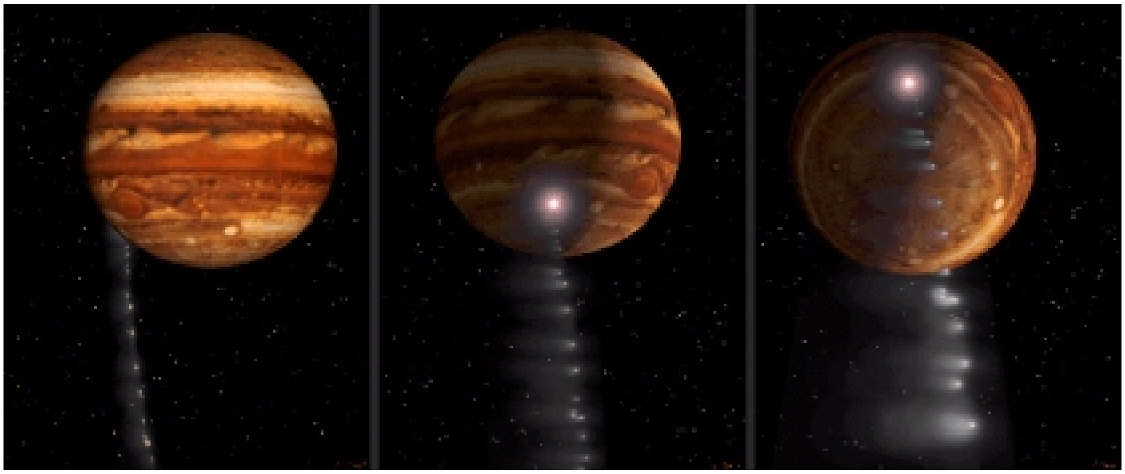


Figure 1. Artist's rendering of a fragmented Shoemaker-Levy 9 impacting Jupiter.

Figure 2. The heavily cratered highlands of the Moon record the period of heavy bombardment that marked the first 500 million years of lunar history.

Figure 3. An aerial view of Meteor Crater, Arizona, one of the Earth's youngest impact craters. Field studies indicate that the crater was formed some 50,000 years ago by an iron mass(es) traveling in excess of 11 km/s and releasing 10 to 20 megatons of energy. The result was the formation of a bowl-shaped crater approximately 1.2 km across and 170 m deep, surrounded by an extensive ejecta blanket.

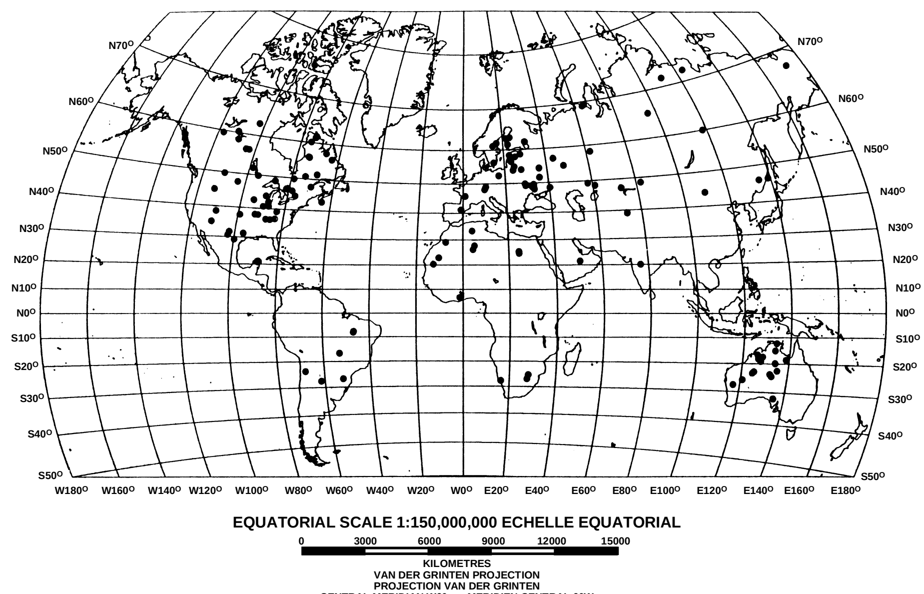


Figure 4. Approximately 130 terrestrial impact craters have been identified. They range up to 140 to 200 km in diameter and from recent to about two billion years in age. More craters have been identified in Australia, North America, and eastern Europe partly because these areas have been relatively stable for considerable geologic periods, thus preserving the early geologic record, and because active search programs have been conducted in these areas.

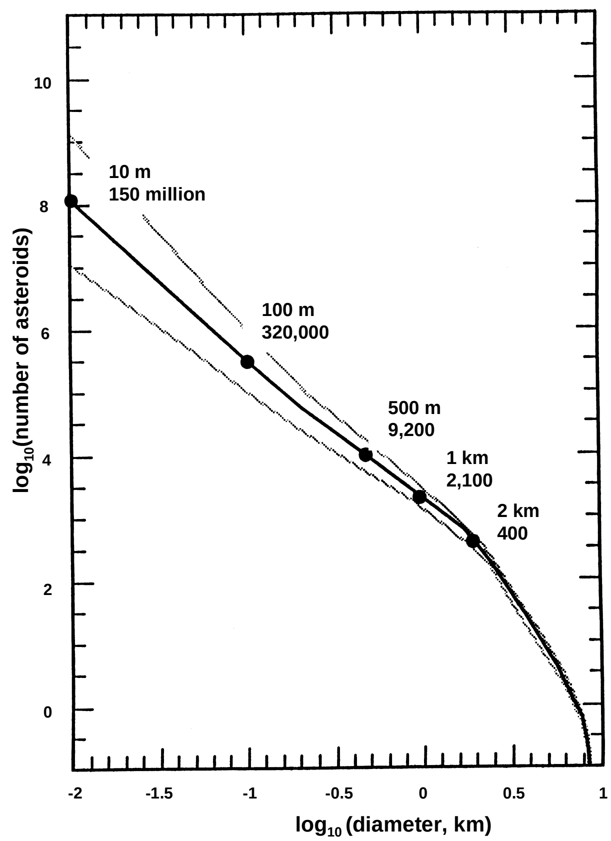


Figure 5. Estimated number of Earth-crossing asteroids larger than a given diameter (E. Bowell).

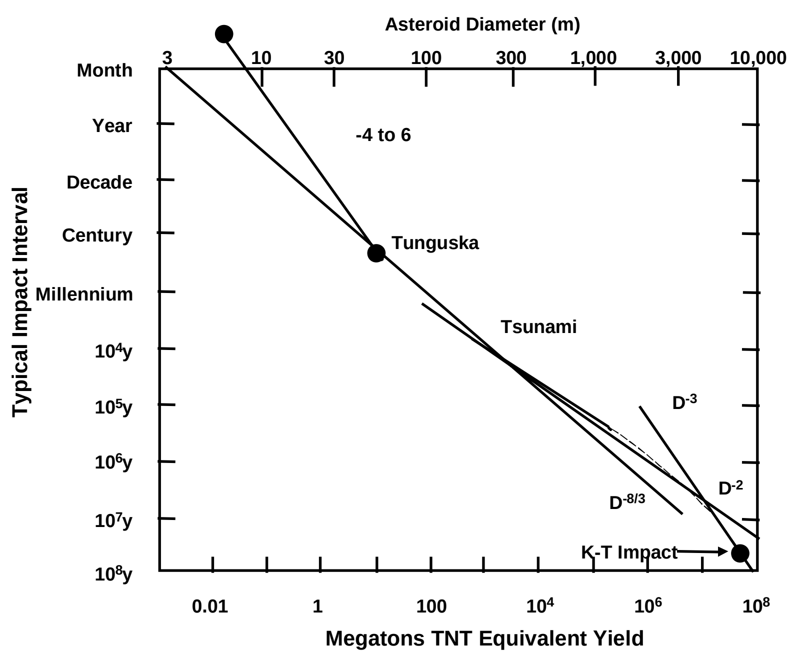


Figure 6. Asteroid impact interval vs. TNT equivalent yield.

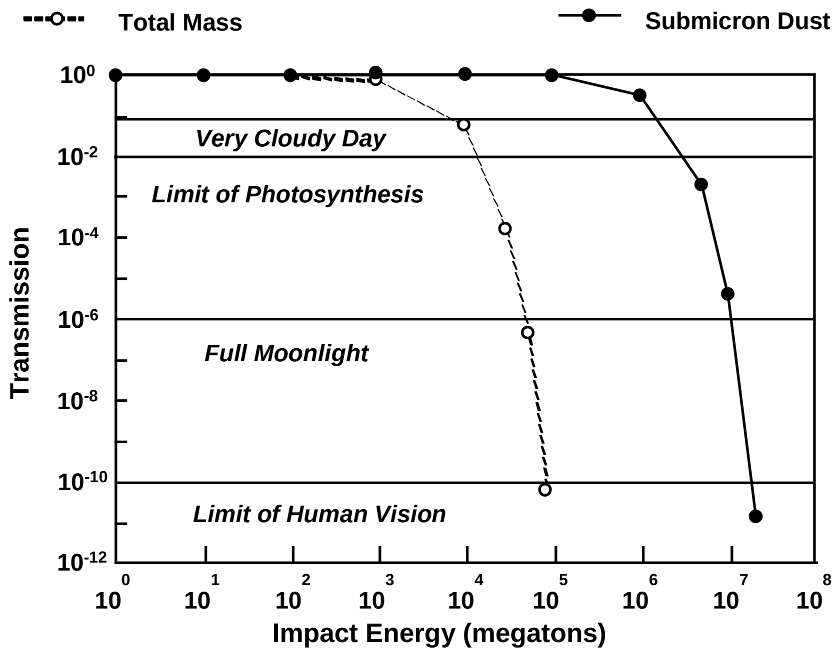


Figure 7. The transmission of light as a function of the kinetic energy of the impacting body assuming either that all of the dust placed in the atmosphere is submicron (total mass), or that only 10 percent of the mass of the impactor (submicron dust) is injected as submicron dust. Also noted in the figure are the light levels that correspond to various natural phenomena.

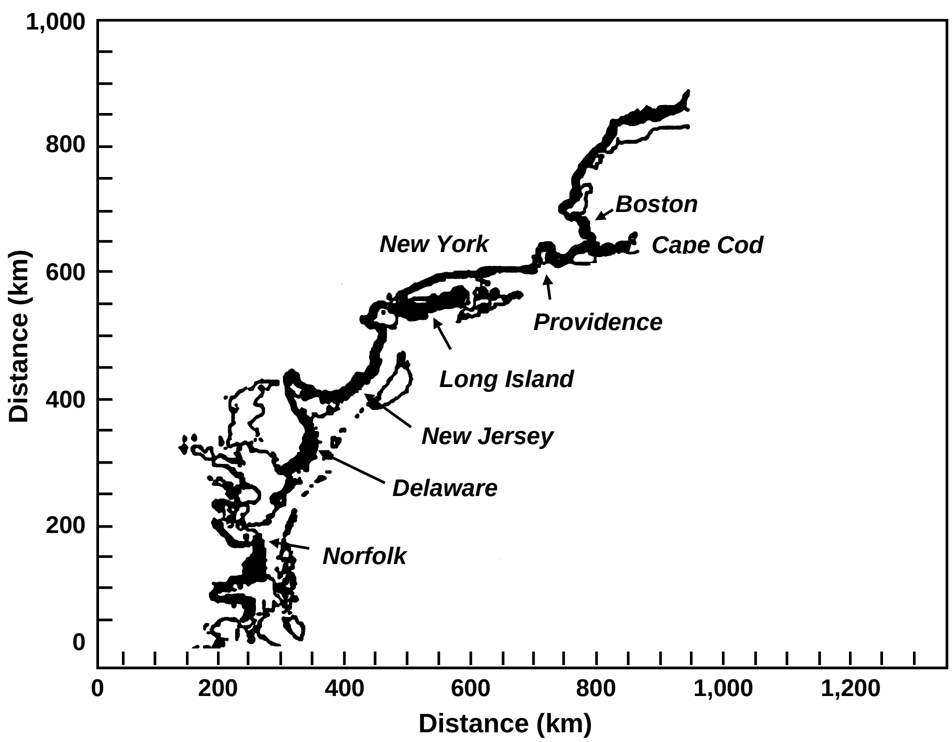


Figure 8. Long Island is swamped. Wave goes 10 to 20 km inland.

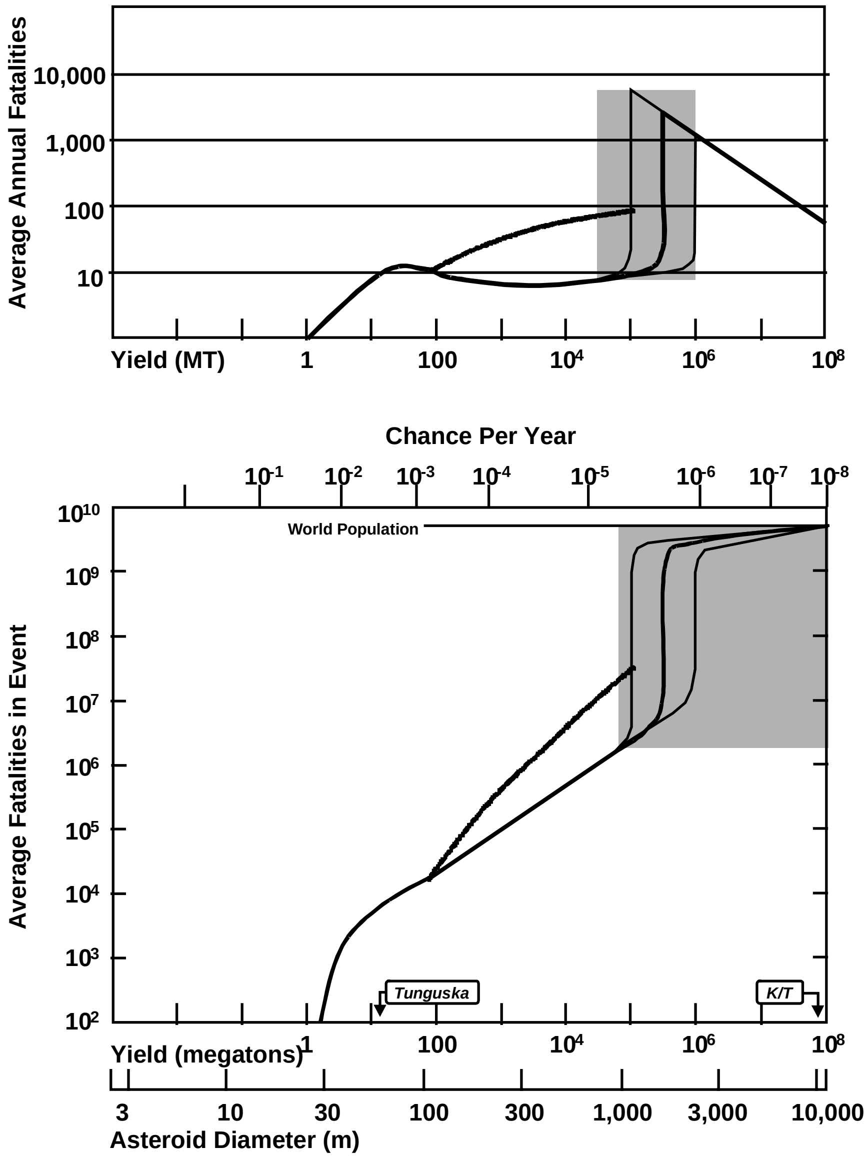


Figure 9. Top: Schematic representation of the average mortality (worldwide deaths per year) from impacts over the same range of energies, showing that the total hazard increases as the size of the impactor ranges from ordinary meteorites up to the global threshold at a nominal energy of 3 x 10^5 MT. Scales for associated impact probabilities and asteroid diameters from Fig. 1 are also shown (figure is adapted from Chapman and Morrison, 1949). Bottom: Average mortality from impacts as a function of energy, for the current population of Earth. The solid line from 10 to 10^5 MT is for impacts on land or the continental shelves; the dashed line indicates increased mortality from tsunami that result from deep ocean impacts. The shaded region shows the range of values associated with the choice of the threshold for global catastrophe (between 10^5 and 10^6 MT).

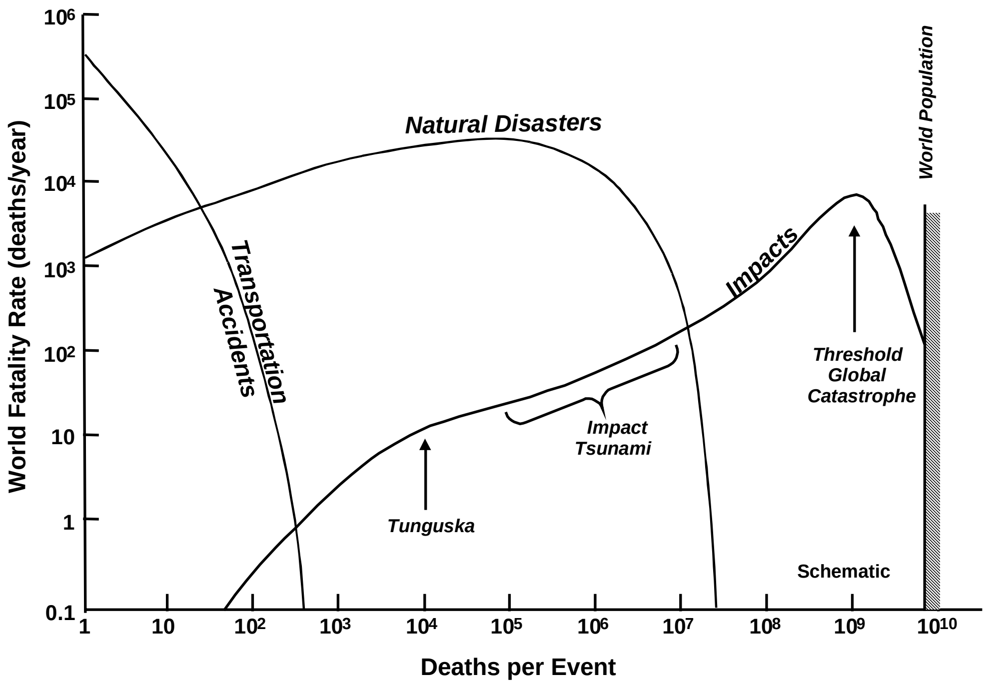


Figure 10. Schematic indication of the fatality rate from accidents and disasters (worldwide deaths per year factor of 10 in the abscissa) as a function of the number of deaths per event. The primary cause of accidental deaths is transportation accidents (including auto, train, and plane), which typically involve the deaths of fewer than 100 persons per incident. Large-scale natural disasters (floods, earthquakes, hurricanes, volcanic eruptions) cause deaths over a wide range of scales, up to rare but statistically important events that can kill millions. Only impacts, however, are capable of killing more than 100 million persons per event, and they dominate the hazard in the right-hand side of the figure.

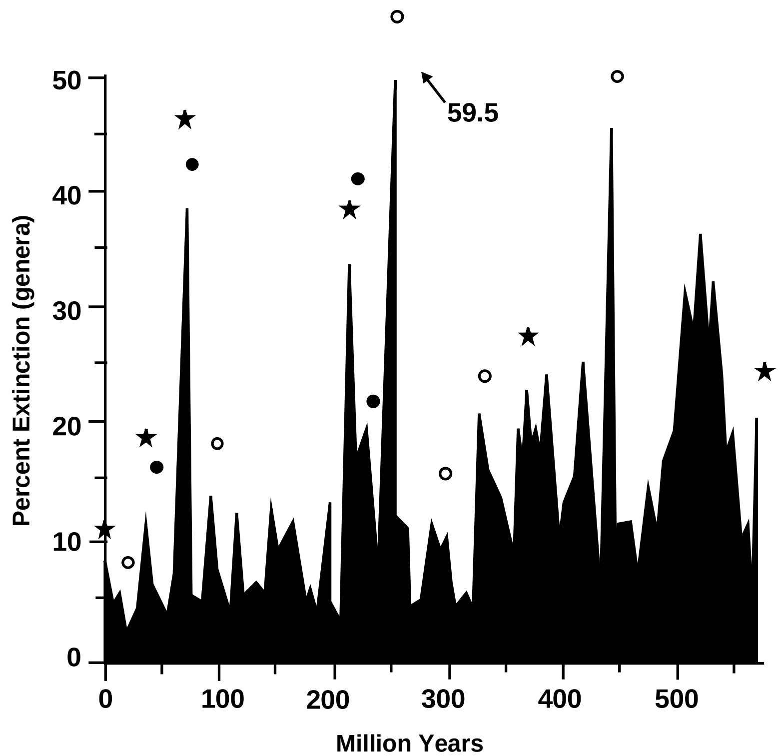


Figure 11. Percent extinction of marine genera per geologic stage (or substage) during the Phanerozoic (data from Sepkoski 1992 and personal communication). Twenty-four local maxima are recognizable in Sepkoski's data; they are as follows (end of stage as dated by the DNAG time scale, or by Bowring et al. [1993] for the Cambrian, in Myr, may not be exactly coincident with the extinctions):

1. Tommotian (530)	*2. Bottomian (520)*	*3. Dresbachian (515)*
4. Trempealeauan (510)	*5. Arenigian (478)*	*6. Caradocian (448)*
7. Ashgillian (438)	*8. Wenlockian (421)*	*9. Givetian (374)*
10. Frasnian (367)	*11. Visean (333)*	*12. Serpukhovian (320)*
13. Stephanian (286)	*14. Guadalupian (253)*	*15. Olenekian (245)*
16. Carnian (225)	*17. Norian (208)*	*18. Pliensbachian (193)*
19. Tithonian (144)	*20. Cenomanian (91)*	*21. Maastrichtian (66.4)*
22. Late Eocene (36.6)	*23. Middle Miocene (11.2)*	*24. Pliocene (1.64)*

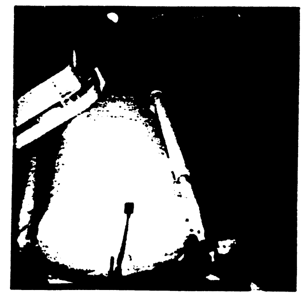
Closed circles: large (greater than 70 km diameter) dated craters
Stars: diagnostic stratigraphic evidence of impact
Open circles: possible stratigraphic evidence of impacts (see text)

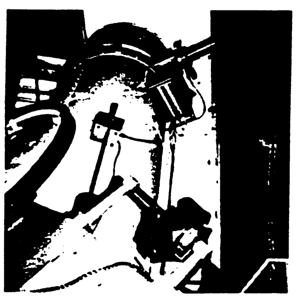
PALOMAR OBSERVATORY, CALIFORNIA:
0.46-m Schmidt

This telescope is already highly productive as a photographic instrument, supporting both the PCAS and PACS programs described in the text. For continued photographic work, the main requirement is in the area of running costs and relatively straightforward instrumental additions. Plans are also under way to convert to CCD detectors.

PALOMAR OBSERVATORY, CALIFORNIA:
1.2-m Oschin Schmidt

This telescope, while currently dedicated to the new northern sky surveys, made significant contributions in the late 1970s to mid 1980s and has potential to make a significant contribution to asteroid searches; no specific plans for asteroid work are in place, however.

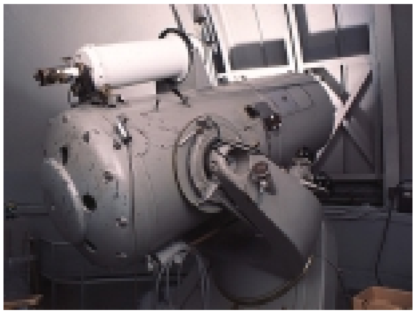




KITT PEAK OBSERVATORY, ARIZONA:
Spacewatch CCD Scanning Telescope

This telescope has a 0.9-m aperture, with plans to upgrade to 1.8 m when funding is obtained. It is used for development of CCD scanning and data reduction techniques as well as the search for NEOs. The 2048 x 2048 pixel CCD, largest in the world, is seen in a liquid-nitrogen cooled down dewar at the top, permanently mounted at the south Newtonian port.

Figure 12. Currently active and potential new programs.



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Photograph by R. Danner and D. Hogg

PALOMAR OBSERVATORY, CALIFORNIA
0.46-m Schmidt

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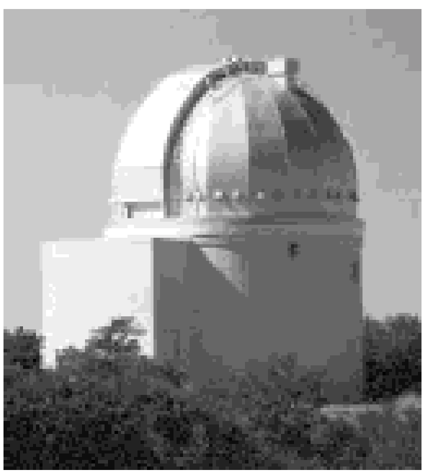

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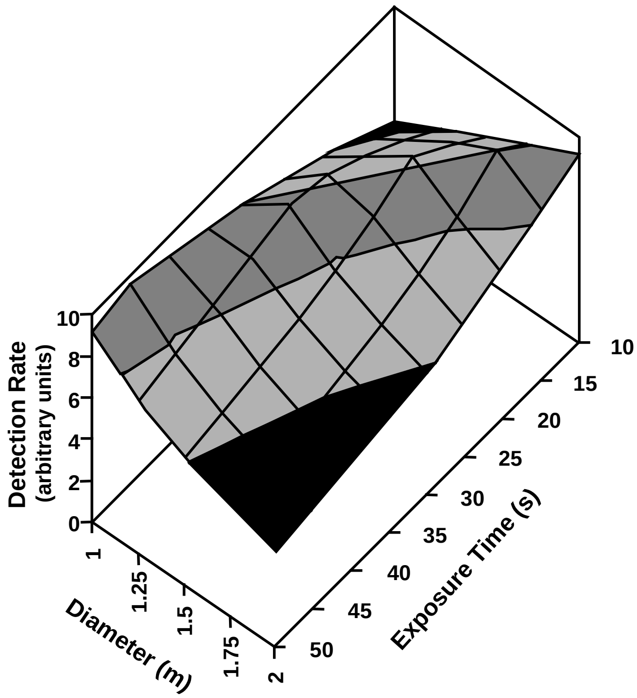

Figure 13. Detection rate versus exposure time for various telescope diameters.

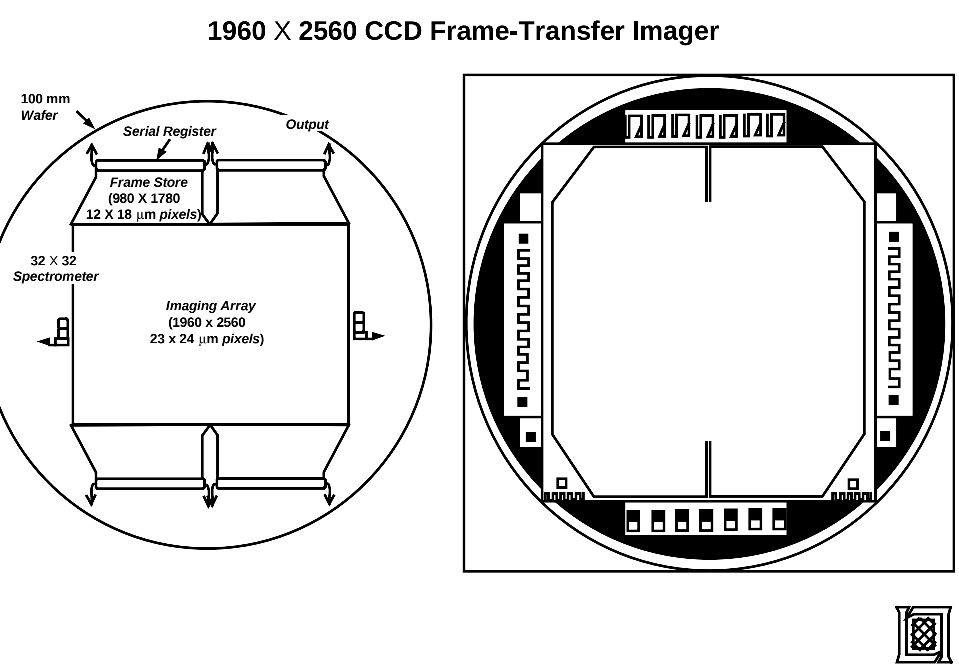

Figure 14. Detail of 1960 x 2560 pixel CCD showing output ports and frame storage locations.

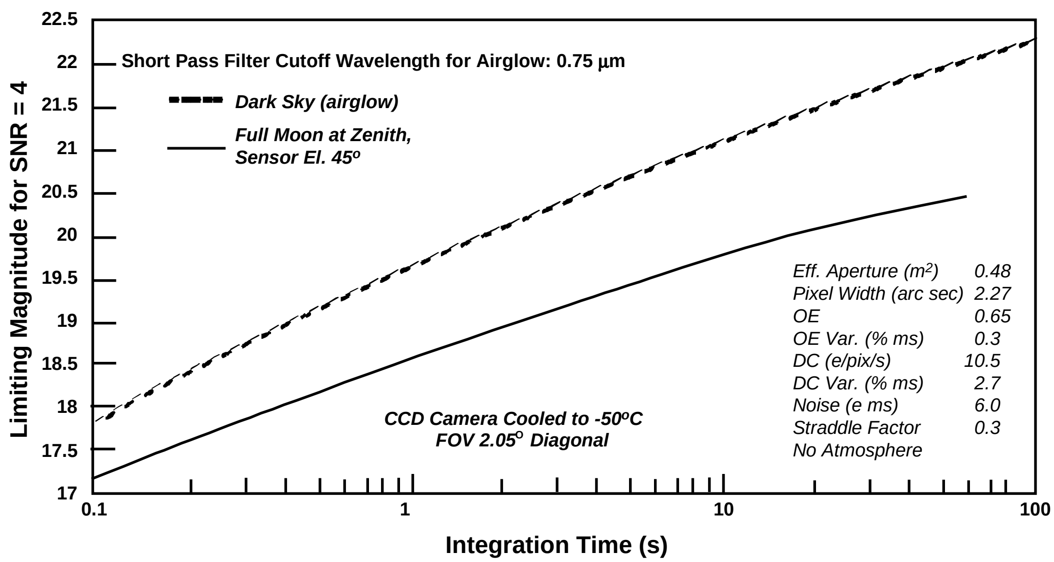

Figure 15. Limiting magnitude achievable using Lincoln CCD detectors on a GEODSS main (1-m) telescope as a function of integration time.

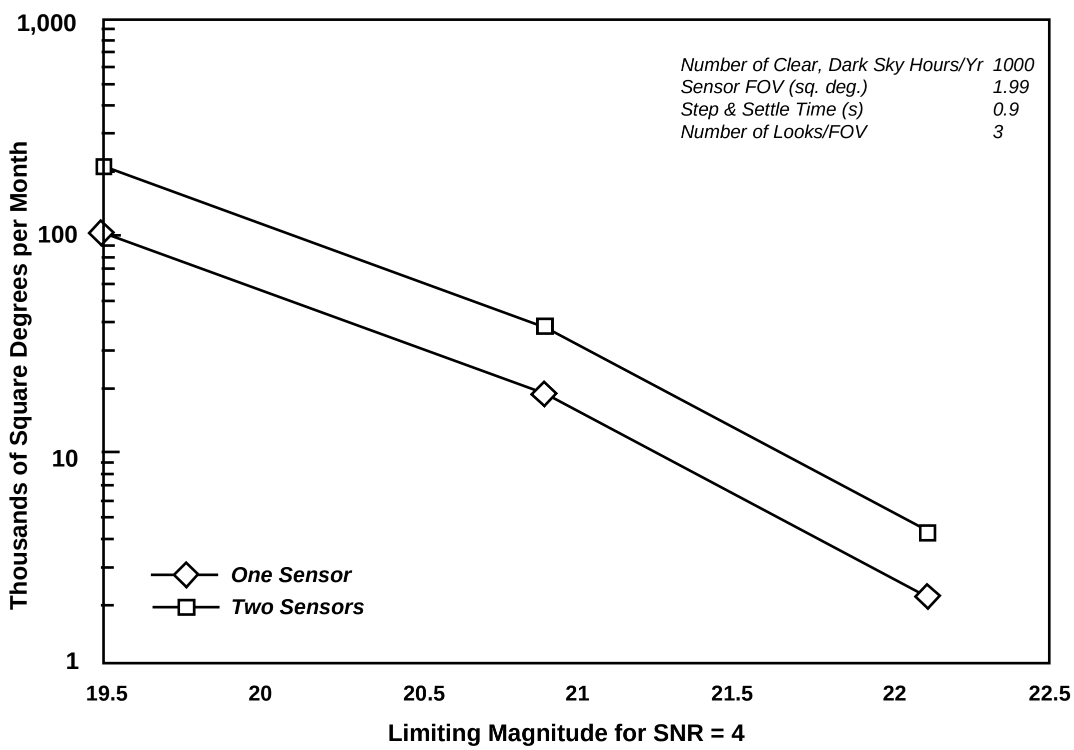

Figure 16. Search rate achievable by one or two GEODSS/CCD telescopes as a function of limiting magnitude.

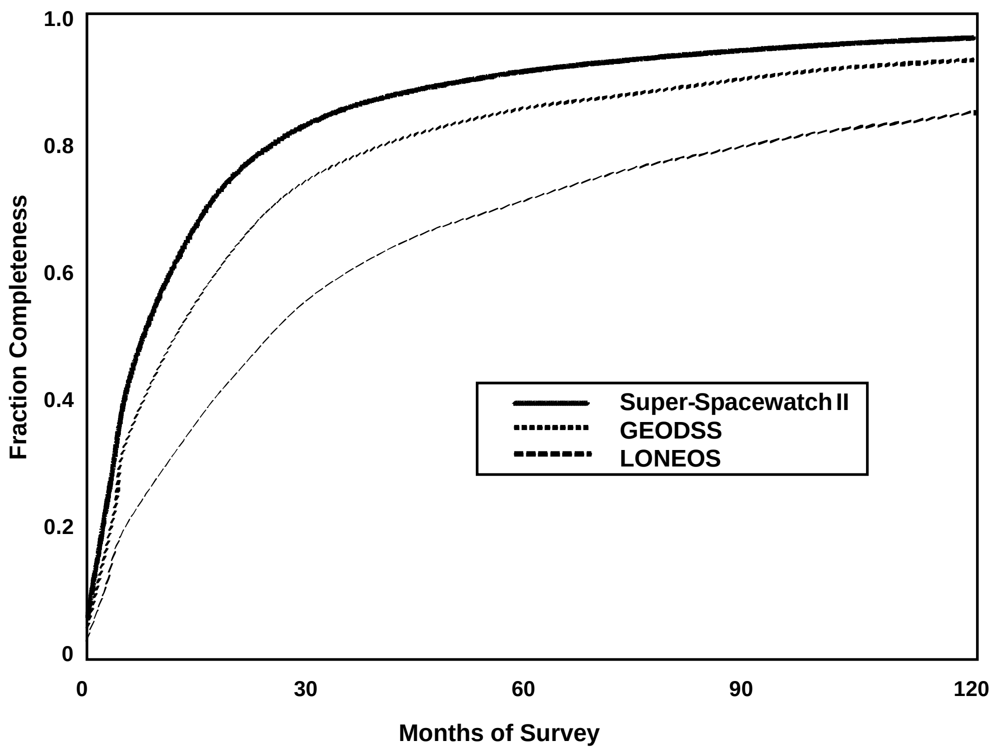

Figure 17. Rate of discoveries vs. time for each of three systems evaluated, assuming that the rate of sky coverage is chosen such that all available sky area is covered each month. The curves represent the discovery rate for objects having either diameters of about 1 km and moderate albedos (0.15) or diameters of about 2 km and low albedo (0.04).

	Space Guard Proposal	Space Watch	GEODSS	TOS	NASA Liquid Mirror	LLNL Wide Field of View
Limiting Magnitude	22-24	20.5	20-22 22 w/ filters	19.3	22	16
CCD Field of View	2º	0.7º	2.1º	2º Estimate	0.35º (CCD ltd)	5.3º
Objective Diameter	2.5 m	0.91 m	1.02 m	0.56 m	3 m	0.10 m
Focal Ratio	5.2	5	2.15	2.4	1.49	3.3
Number of Optical Telescopes	6	1	2	1	~10	9
Coverage (Sky Region)	1000 sq deg per mo per telescope	~2700 sq deg per mo	2958 sq deg per mo per telescope	Similar to GEODSS 2958 sq deg per mo per telescope	10.5 sq deg per hour per scope	2500 sq deg per night per scope
Number of Scans per Sky Region	9	1	9	2 or more per scope	2 with 10 scopes	More than 9
Resolution	1 arc sec	1 arc sec	2.27 arc sec	Assume GEODSS 2.27 arc sec	1 arc sec	9.3 arc sec (Loral Unit)
Total System Coverage Note 58	6000 sq deg per mo	~ 2700 sq deg per mo	5917 sq deg per mo	2958 sq deg per mo	6300 sq deg per mo	22,8000 sq deg per mo
Time Required to Find 90% of Asteroids & Short-Period Comets, 1 km and Larger	25 yrs	~50+ yrs	~25 yrs	~50+ yrs	~25 yrs	Only 28% at 25 yrs
Maximum Warning Time for 1-km Asteroid; Albedo = 0.03 F(a) = 0	9 months at magnitude 22, 1.3 years at magnitude 24	6 months	9 months	6 months	9 months	3 months

Figure 18. Optical search system specifications.

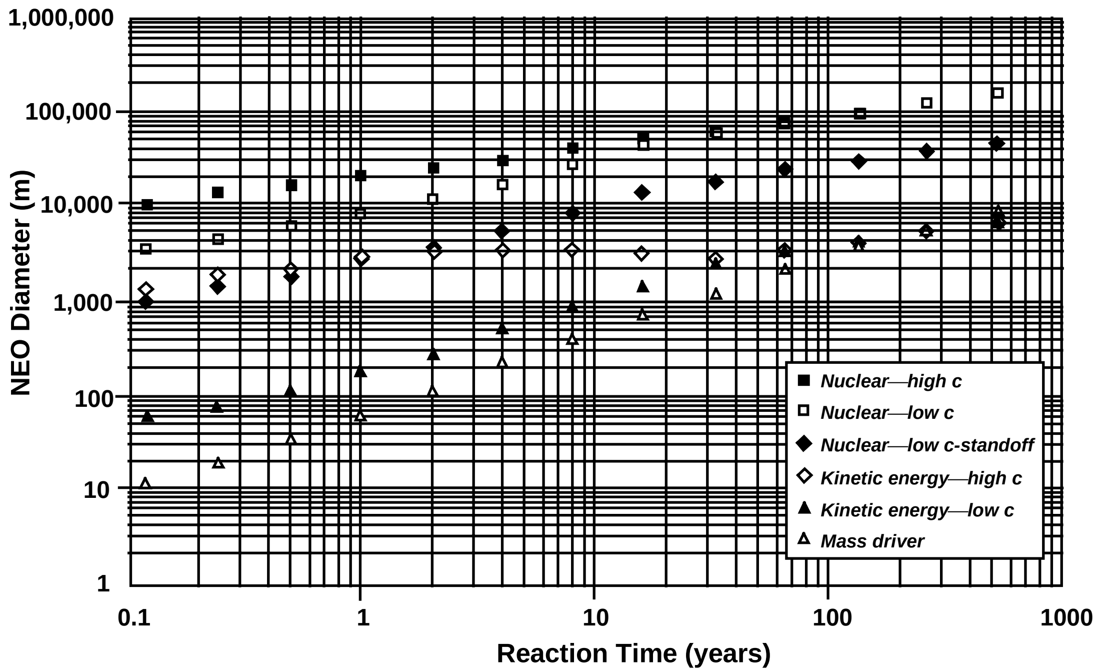

Figure 19. Maximum NEO diameter that can be deflected as a function of reaction time.

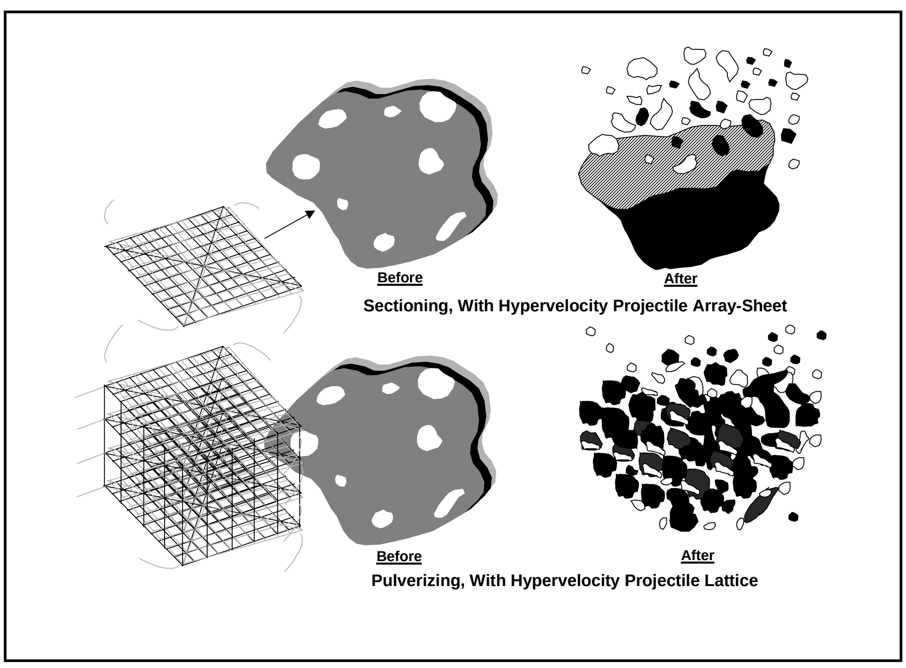

Figure 20. Threat object pulverization.

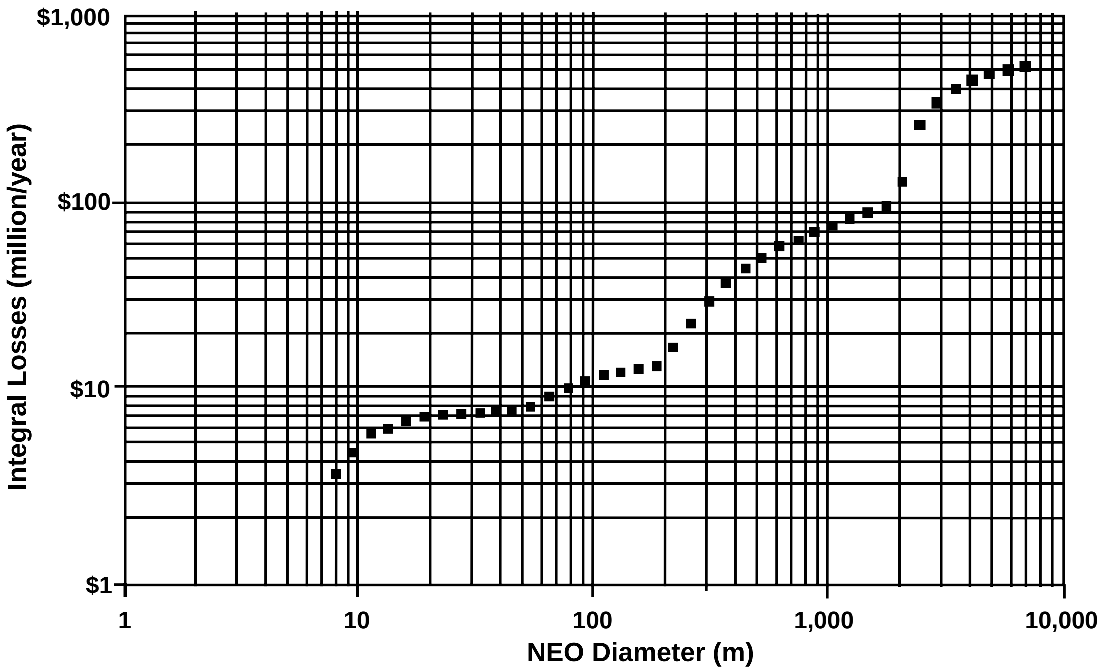

Figure 21. Losses integrated over diameter.

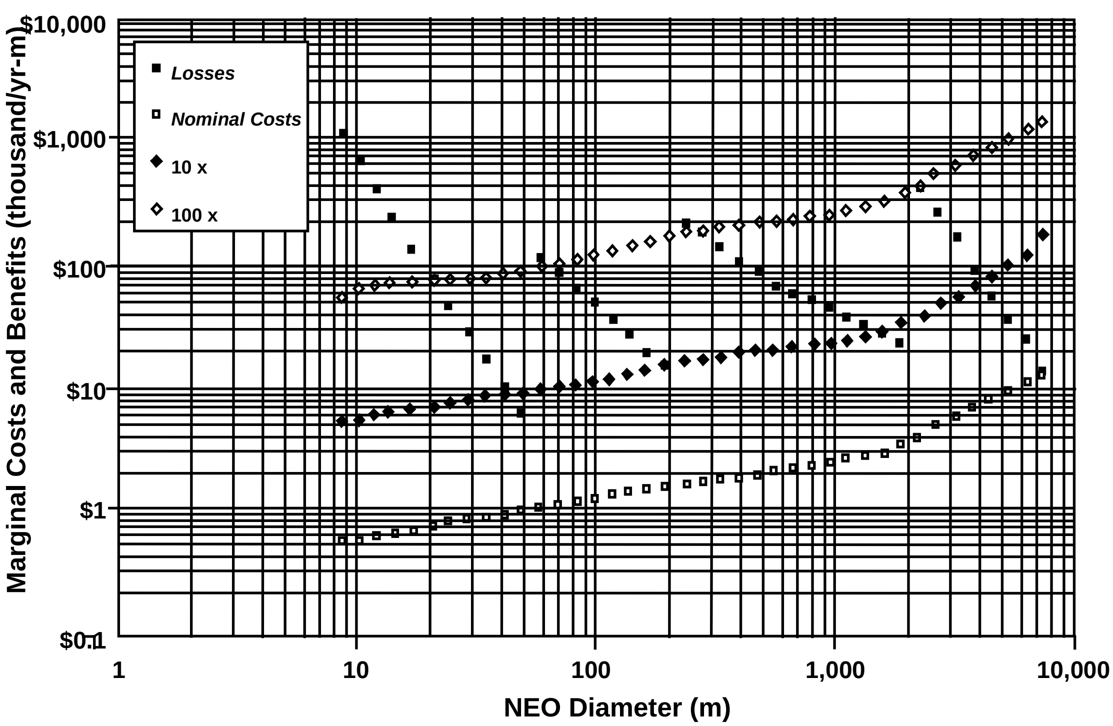

Figure 22. Marginal costs and benefits of NEO defenses.

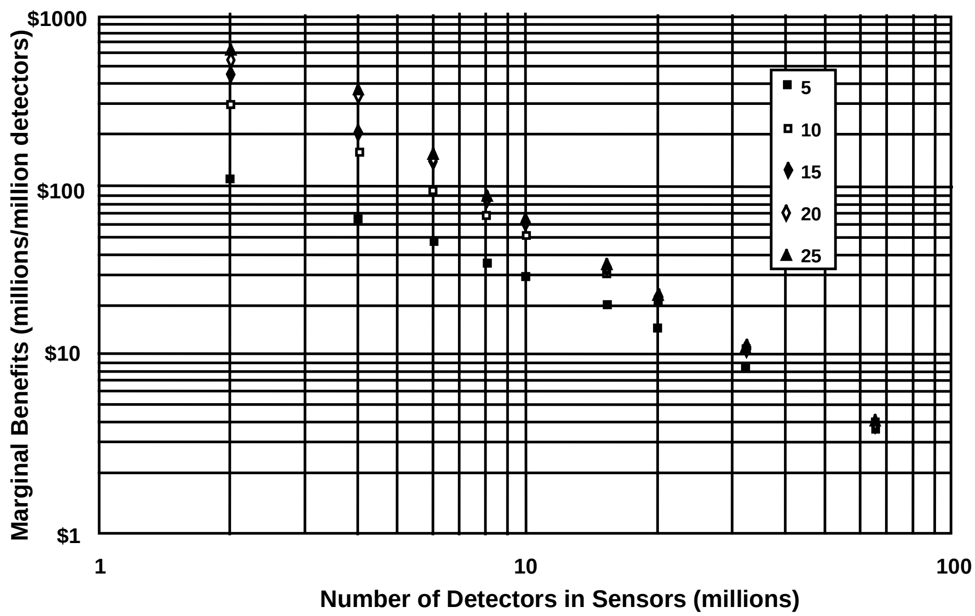

Figure 23. Marginal costs as a function of the number of detectors and search time.

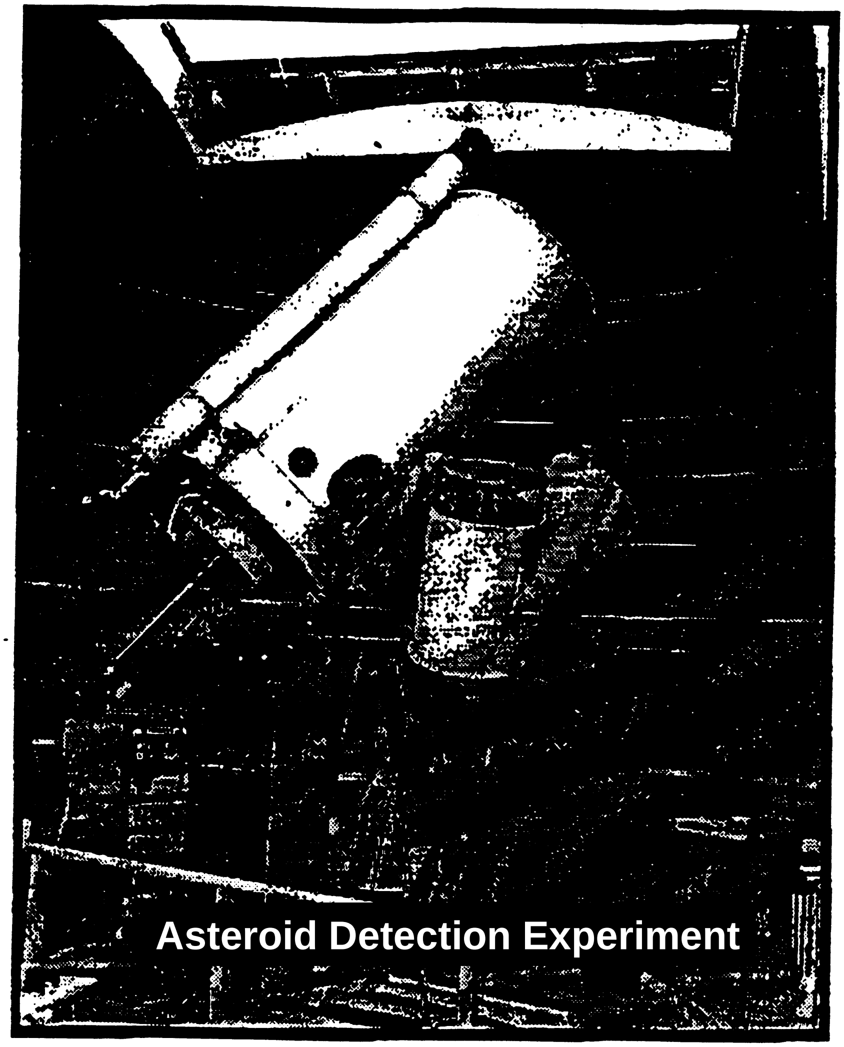

Figure 24. Experimental test system telescope used during historical field tests, including asteroid detection experiments. Figure shows the 31-inch polar mount Cassegrain telescope at Lincoln Experimental Test Site.

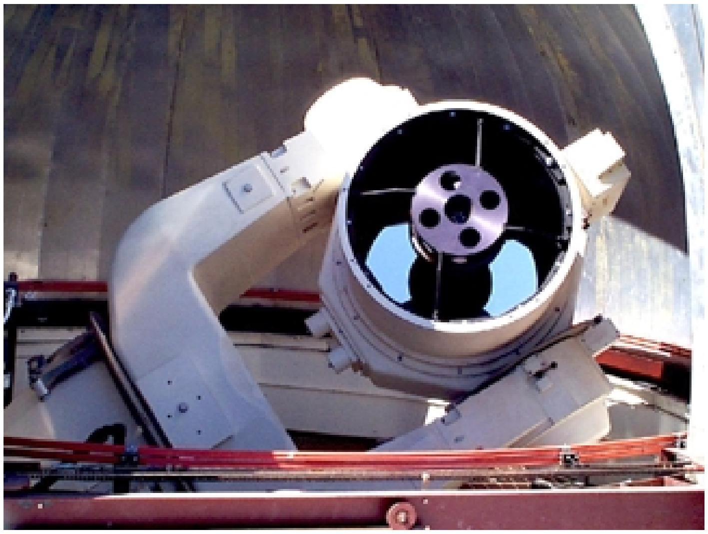

Figure 24. *Experimental test system telescope used during historical field tests, including asteroid detection experiments. Figure shows the 31-inch polar mount Cassegrain telescope at MIT Lincoln Laboratory Experimental Test Site.*

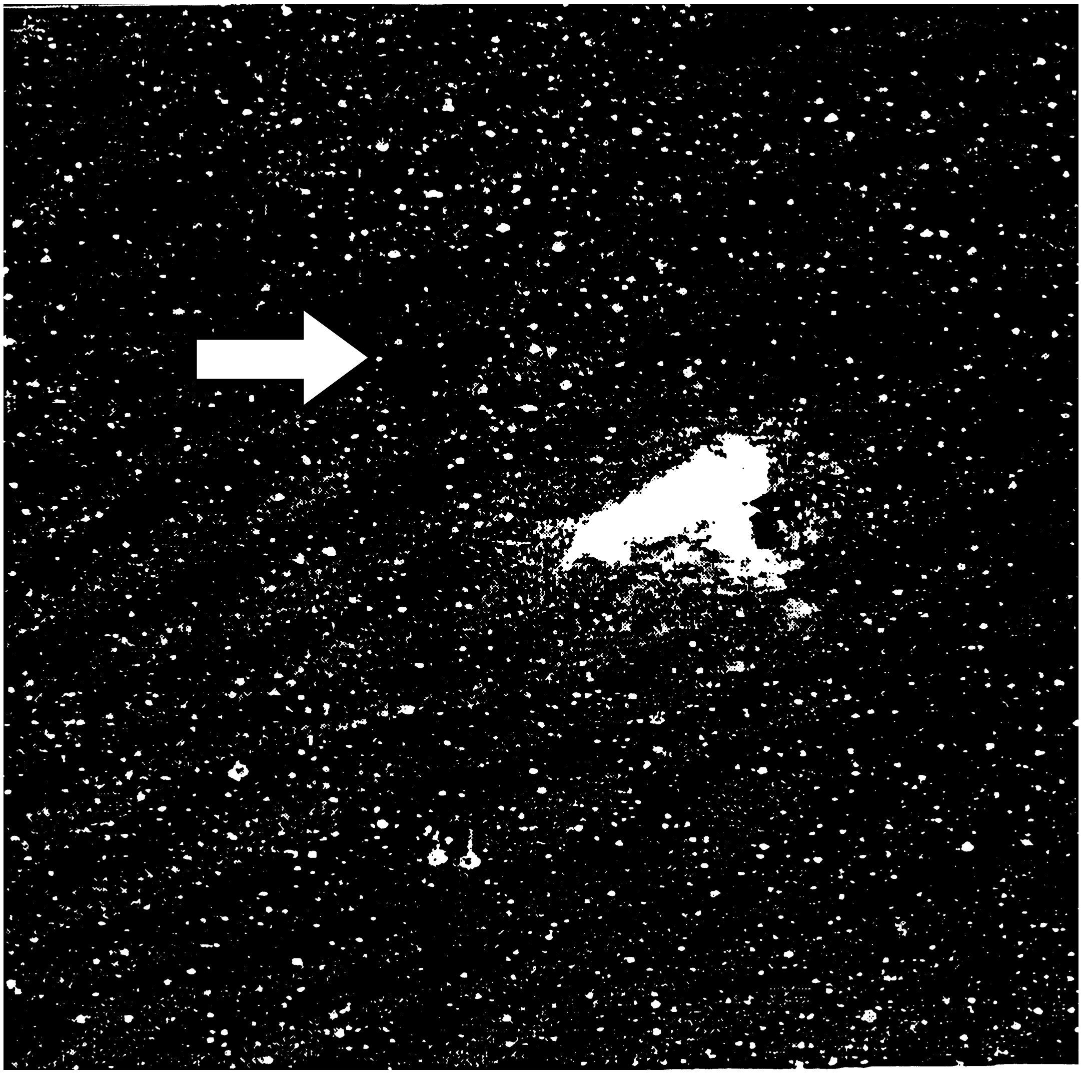

Figure 25. Detection of asteroid 114 Kassandra near the Omega Nebula.

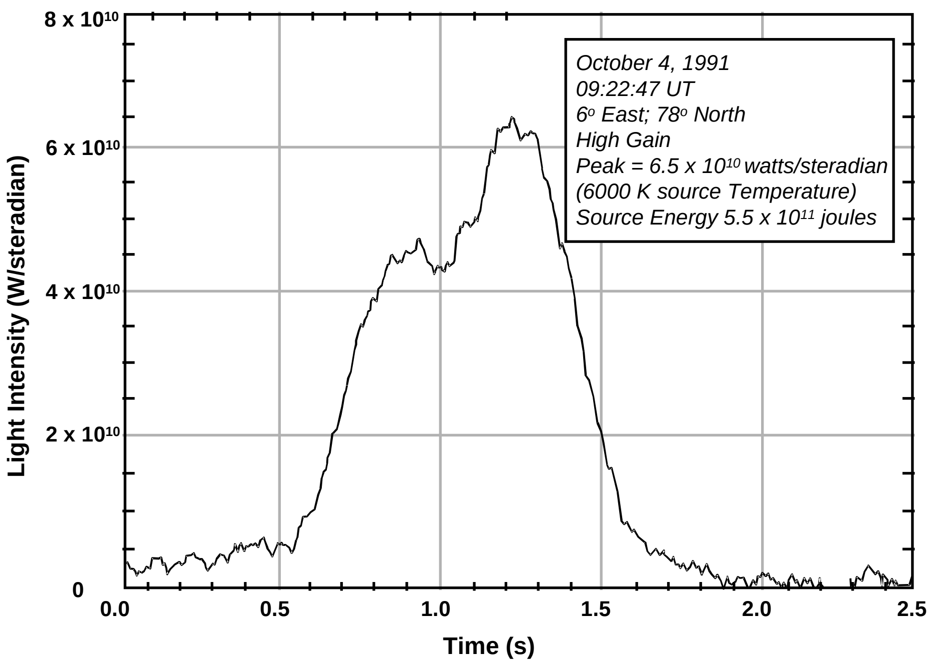

Figure 26. Visible light curve from the meteoroid impact of October 4, 1991. Presented is the visible light intensity (watts/steradian) vs. time (seconds).

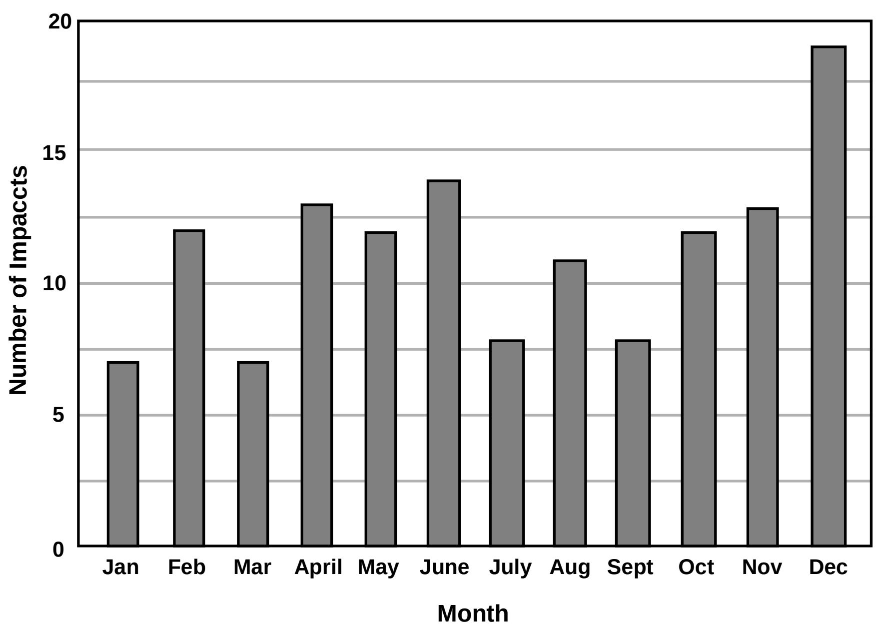

Figure 27. Impacts per month between 1975 and 1982.

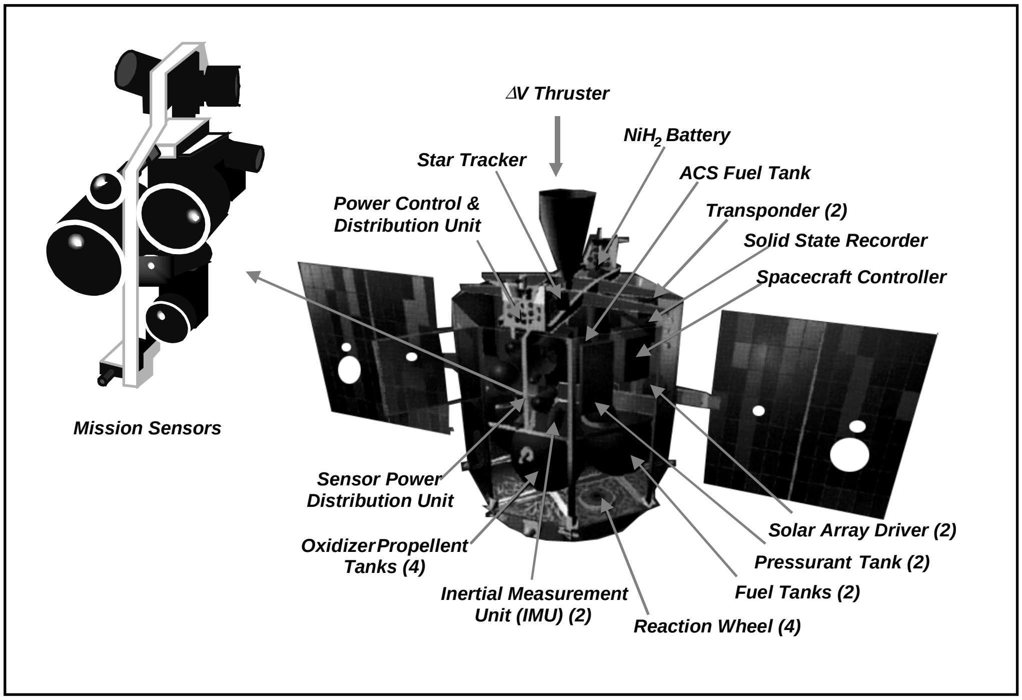

Figure 28. Clementine internal layout.

Long Warning	**1995**	**2000**	**2005**	**2010**	**2015**
Search	→	→	50% →	→	90%
Intercept	Current →	→	Maintain →	→	Decision
Improved	Study →	→	Develop →	→	
Short Warning	**1995**	**2000**	**2005**	**2010**	**2015**
Search	Study →	→	Develop →	Operate	
Search Space	Study →	→	Develop →	→	Deploy
Interact Exp.	Plan →	→	Execute →	Complete	
Intercept Tech.	Plan →	→	Test →	→	Residual
Very Short Warning		**2000**	**2005**	**2010**	**2015**
Search Ground	Plan →	→	Modify/Deploy		
Search Space	Plan →	→	Modify/Deploy		
Interact Exp.	Plan →	→	Complete		
Intercept Tech.	Plan →	→	Residual		

Figure 29. Strawman programs.

- **Threat from intermediate and large objects significant**
  - **Means exist to detect and address each**
  - **Laboratory and space experiments are defined to validate predictions**
  - **As warning is reduced, so are options, but warning is adequate for important intermediate objects**
- **Defensive alternatives depend most strongly on object size and warning time**
  - **Long Warning objects observed many orbits prior to impact—search only does not lead to defenses; it addresses only a portion of the threat**
  - **Short Warning defenses are ready, but there are development, cost, and delay issues**
  - **Very Short Warning defenses against the most likely part of the threat are possible soon**
    - **Address using nonthreatening technologies with growth potential**
    - **Common ground/space-based sensors, interaction and intercept experiments**
- **Long and Very Short Warning programs together would cost about $100 million/year**
  - **Address both large, short-period objects and intermediate objects on final approach**
  - **Develop interceptor and control technologies for others**
- **Experiments over one to two decades evaluate interceptor/control cost-effectiveness**
- **Combination of studies, laboratory and space experiments, and intercept technology**
  - **Performed openly, with scientific and international cooperation**
  - **Refine estimates and assess effectiveness/affordability of all defenses in decade**

Figure 30. Summary.

Appendix A

Task Statement

Space Surveillance, Asteroid and Comet Impact Warning for Earth, and Space Debris

March 1995

Space Surveillance

Background. The Space Surveillance mission has been handled by the Air Force since 1957 when the first Sputniks were launched. The initial facility was at Hanscom AFB and was later moved to the Cheyenne Mountain Complex in Colorado Springs. The mission requirements were largely driven by the Soviet threat for all these years. In fact, the missile warning mission has dominated the space surveillance mission to such an extent that the evolution of capability in the latter has been painfully slow and has lagged the state of the art substantially.

The space surveillance mission area remains an essential part of the Air Force function to support the warfighter with space assets. The threat to space assets and their supporting capability is evolving with the need to monitor an increasingly crowded environment. Operational spacecraft have in general been large objects easily tracked by the space surveillance network.

The most serious problem with the current system is that the theory, software, and hardware used for orbit determination at Space Control Center (SCC) have evolved only slowly over the last 20 years, while the state of knowledge of orbit determination, the state of software and hardware technology, the sensitivity of sensors, and the accuracy of the data have advanced immensely. This has precluded the system from taking advantage of the accuracy of the data to reduce the overall tasking load of the sensors, which at the same time would enable them to contribute more in other areas of space surveillance, such as debris monitoring, and consequently address more areas for the same total cost.

The sensitivity of several sensors has increased significantly in the last 20 years. This has substantially enhanced the number of objects detected. However, processing limitations at SCC have precluded the maintainable catalog from absorbing all these new objects into the data base.

In future applications of surveillance data, improved accuracy and the ability to define that accuracy in meaningful terms to the warfighter will be a primary objective. The accuracy of sensors has increased substantially over the last two decades, but the capability to calibrate these instruments on-line to their inherent noise level is only now becoming available with laser instrumentation. The remaining impediment to achieving higher accuracy is the drag environment for satellites operating below 1,000 km, and this will only be resolved when on-line procedures for calibration of the density models are implemented. Finally, as increasing demands on accuracy are made by the user community, alternative filter technologies that can produce covariance products incorporating both the sensor and environmental error models should be considered.

Task Description. The Committee should

- Assess the capability of the current Space Surveillance Networks (SSN) with respect to search for new or lost earth satellites or objects, accuracy of measurements, timeliness, and transmission of sensor data to a central catalog station for all altitudes from 150 km to 35,000 km. Include considerations of reducing errors.
- Determine what and how improvements to the SSN should be accomplished.
- Assess the capability of the current earth satellite catalog production with respect to accuracy, timeliness, and dissemination of data products. Include analyses of environmental factors that introduce errors into catalog products and of technologies that define the propagation of these errors into catalog products with high confidence.
- Determine what and how improvements in producing the catalog(s) can be accomplished. Consider the exploitation of computer performance as an opportunity to transition the catalog to a format based upon special perturbations technology with trusted covariance properties embedded into it.
- Assess the benefits of improved accuracy and of the ability to define that accuracy in meaningful terms to Air Force, interagency, and international operations as orbits with desirable properties (e.g., sun synchronous) are exploited by an increasing number of spacefaring nations.
- Recommend appropriate Air Force actions.

Asteroid and Comet Impact Warning for Earth

Background. The growing concerns about the asteroid and comet threat to earth may result in a new mission for the space surveillance system. The capability to integrate and perform this potential mission needs to be assessed as part of the future architecture of space surveillance.

Asteroids and comets have struck the earth over its history in Russia, Yucatan, and the United States (Arizona). It is now believed that an asteroid impact caused the cataclysmic extinction of the dinosaurs. Although impacts are rare, they could have devastating effects. At a minimum the Air Force should consider Deep Space Surveillance as a new mission area.

Task Description. The Committee should

- Review and assess the Asteroid and Comet environment and earth impact rate
- Assess detection and tracking requirements and Air Force capabilities
- Determine and describe appropriate capabilities and missions for the Air Force
- Recommend Air Force actions for these new missions

Space Debris

Background. There is a proliferation of smaller-size satellites on one hand and a large, uncontrolled growth of debris, consisting of dead satellites and fragments from breakups of a variety of sizes, on the other. As a result, there is a significant overlap of the two. Further, there is a growing national concern, driven by the National Aeronautics and Space Administration's (NASA's) requirements for keeping track of debris down to 1 cm characteristic size for safety of manned spacecraft. Hence, the space surveillance system must maintain an orderly and accurate catalog of all objects in space to ensure that the mission is accomplished despite its evolving nature.

The only organized collection and analysis of small Space Debris has been by NASA/JSC, employing modeling and estimates because of the sparse data that have so far been collected. Further, the numbers of objects in the Air Force catalog and NASA's debris curves do not agree where they overlap in the 10- to 100-cm object size range. The Air Force should consider a more active role.

Further, the Air Force has established debris mitigation procedures that should be reviewed for their adequacy.

Task Description. The Committee should

- Independently assess the seriousness of space debris as it may affect Air Force assets and space operations.
- Evaluate the dynamics and factors that produce and/or reduce space debris.
- Independently review the models and assumptions for the evolution of the historical space debris calculations and predictions, particularly for the condition of unstable growth known as collisional cascading.
- Review and compare Air Force studies measurements, assumptions models and assessments of space debris with those produced by other government agencies. Determine the reasons for any differences.
- Recommend appropriate Air Force actions.

Potential Impact of the Study

The major result of the study would be to identify means to enhance overall mission capabilities of the Air Force in the three subjects addressed substantially while reducing operational costs of the system.

The reduction of the manpower and the reduced number of sensor sites required for Space Surveillance, the reduction of the maintenance of the software by using more commercial packages, and minimizing the use of one-of-a-kind software/hardware packages in application will be clarified.

The actions required by the Air Force in the new mission area of Planetary Defense will be identified.

The seriousness of Space Debris and its effects on Air Force space operations will be clarified and any additional efforts required will be identified.

Study Organization

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General Officer Participant	Brig Gen Thomas J. Scanlan, Jr., SAF/ST
Senior Civilian Participant	Mr. John H. Darrah, HQ AFSPC/CN
Executive Officer	Lt Col Donald Jewell, HQ AFSPC

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Appendix B

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Executive Vice President and
General Manager
Washington Operations
Science Applications International
Corporation

Dr. Edward Teller
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Lawrence Livermore National Laboratory

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Col Simon P. Worden, USAF
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Chief Scientist
HQ Air Force Space Command

Brig Gen Thomas J. Scanlan, Jr., USAF
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* *Affiliations as of 1 March 1995*

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Dr. Greg Canavan
Mr. Ted Jarvis
Dr. Dean Judd
Dr. Tony Pensa
Mr. Sam Tennant
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Space Debris Panel
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Asteroids and Comets Panel
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Dr. Dean Judd
Dr. Bob Naka
Dr. Edward Teller

At-Large Members
Dr. Randy Clinton
Maj Gen Rosie Rosenberg, USAF (Ret)
Col Pete Worden, USAF

Appendix C

Committee Meetings

Full Committee Meetings

- MIT Lincoln Laboratory, Lexington, MA 8-10 May 1995
- MITRE Corporation, McLean, VA 30 May to 2 June 1995
- HQ AFSPC, Colorado Springs, CO 19-21 June 1995
- Loral Aeronutronic, Santa Margarita, CA 17-19 July 1995
- MITRE Corporation, Colorado Springs, CO 18-20 September 1995
- Phillips Laboratory, Albuquerque, NM 9-11 January 1996
- MIT Lincoln Laboratory, Lexington, MA 20-23 February 1996

Surveillance Panel Meetings

- MITRE Corporation, Colorado Springs, CO 28-29 November 1995
- Space and Missile Systems Center, Los Angeles, CA 14 March 1996
- Cape Cod AFS, MA 19 March 1996

Asteroids and Comets Panel Meetings

- Lawrence Livermore National Laboratory, Livermore, CA 22-26 May 1995
- Many installations, Maui, Hawaii, and Oahu, HI* 8-12 April 1996

Debris Panel Meetings

- Phillips & Sandia Laboratories, Albuquerque, NM 11 August 1995
- NASA Johnson Space Center, Houston, TX 16-17 August 1995
- NASA Johnson Space Center, Houston, TX 17 October 1995

* *Joint meeting with Surveillance Panel*

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Appendix D

Acronyms and Abbreviations

AFSPC	Air Force Space Command
AU	astronomical unit
CCD	charge-coupled device
DoD	Department of Defense
FASTT	Fragmentation Algorithm for Strategic and Theater Targets
GEODSS	Ground-Based Electro-Optical Deep Space Surveillance
J	joule
kg	kilogram
km	kilometer
LLNL	Lawrence Livermore National Laboratory
LONEOS	Lowell NEO System
LPC	long-period comet
m	meter
MIT	Massachusetts Institute of Technology
MT	megaton
NEO	near-Earth object
s	second
SCC	Space Control Center
SNR	signal-to-noise ratio
TOS	Transportable Optical System
yr	year

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Appendix E

Initial Distribution

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SAF/OS Secretary of the Air Force

AF/CC	Air Force Chief of Staff
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SAF/AQ	Military Director, USAF Scientific Advisory Board
SAF/SX	Deputy Assistant Secretary for Space Plans and Policy (2 copies)
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AF/SC	Deputy Chief of Staff, Communications and Information
AF/ST	Air Force Chief Scientist
AF/XO	Deputy Chief of Staff, Air and Space Operations
AF/XP	Deputy Chief of Staff, Plans and Programs

Air Force Space Command

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Air Force Materiel Command

AFMC/CC	Commander
AFRL/CC	Commander, Air Force Research Laboratory
AFMC/EN	Directorate of Engineering and Technical Management

National Reconnaissance Office

Director

Other

AF SAB Co-Chairs

Study Committee

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13. ABSTRACT (Maximum 200 Words)

This Study was produced by the Air Force Scientific Advisory Board. It was requested by the Commander Air Force Space Command and approved by the Secretary and Chief of Staff of the Air Force. It covers three topics, each of sufficient depth to be study of its own: Space Surveillance, Asteroid and Comet Impact Warning for Earth, and Space Debris.

Although the Committee's recommendations cover only the surveillance portion (detection, tracking, and cataloging of asteroids and comets, and perhaps size and rotation) of Planetary Defense, this volume presents the entire subject for the sake of completeness.

The impact 58 million years ago at Xhichulub on the Yucatan peninsula is now believed to have ended the age of the dinosaurs.

For Asteroid and Comet warning of impact on earth, the Committee recommended that the Air Force play an active role employing the optical sensors it has deployed around the world.

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